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LIFE

AND THE

EQUIVALENCE OF FORCE.

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PART I.

HISTORICAL NOTICE OF THE DISCOVERY OF THE LAW OF EQUIVALENCE OF FORCE.

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CHAPTER I.

INTRODUCTORY AND HISTORICAL.

In the course of the series of articles on specifics, lately written by me, I have been forced to refer to Fletcher's principles of physiology and pathology,* as the only basis for a rational explanation of many difficult problems. These works are now little, if at all, known in the medical world, no doubt, partly by reason of the fragmentary nature in which they now exist, owing to the untimely death of the gifted author, which thus left the exposition of a large part of his principles to the imperfections of posthumous editing. Since their publication the progress of physiological science in matters of detail has been immense, and, therefore, no general work which does not include all these will be looked upon otherwise than as antiquated. Although the same may not apply to general principles, yet these must be shown to be in harmony with any subsequently discovered law or great principle which may largely affect the sciences of physics and chemistry. Such is the law of the conservation and equivalence of force, and unless Fletcher's doctrines of the nature of life, and the important part played by stimuli be brought into harmony with it, they will find no acceptation. The existence of such harmony must be declared by a living voice, as we can hardly expect a presumedly antiquated book to speak for itself to the uninterested general reader. I had hoped that ere now some fully qualified professed physiologist would have stepped forward to perform this duty, but, having waited in vain for many years, it seems to devolve upon me as the sole surviving editor of any part of Fletcher's works, though I am conscious that such a task can be but imperfectly accomplished by a physician in active practice, whose scanty hours of leisure are perpetually exposed to interruption.

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* *Rudiments of Physiology, Elements of Pathology.* Maclachlan and Stewart, Edinburgh.

The first step was, of course, to make myself fully acquainted with the nature of the doctrine of conservation of force, and its bearing on physics and chemistry; but on approaching the subject, I found no compendious and complete history and exposition of the doctrine in our language adapted to the general run of medical men.

In fact, the doctrine itself is in process of evolution, and the data are scattered in the original memoirs of the men who are now engaged in working it out, chiefly in this country and in Germany. Even in the most recent compendious treatises on physics and chemistry, we can scarcely say that it is fully treated of by itself, though in Brooke's 'Natural Philosophy' and the last edition of Fownes's 'Chemistry,' it is incorporated with the subjects in detail, which cannot be said of the majority of such works.

The same may be said of works on physiology, although in Carpenter's seventh edition, there is a chapter on the balance of the vital economy, in which it is taken for granted that the reader understands the principle. This is taking a good deal for granted.

Grove's classical work on the correlation of the physical forces will at once suggest itself as sufficient; but it is not so, as that work is addressed to readers having a more profound preliminary knowledge of physics than most of our profession can boast of, in fact, it shoots over our heads; and, besides, it does not extend the law to the domain of physiology. The same applies to the treatises of Brooke and of B. Jones and Watt's edition of Fownes, and Tait's 'Thermodynamics.' It is true B. Jones's 'Croonian Lectures on Matter and Force,' being addressed to medical men, might be expected to supply the want.

But this work, besides presupposing too much preliminary knowledge in the reader, contains, we think, erroneous views on some very important points, to contest which would require reference to much information not contained in it, and the sources of which may not be open to the reader. Therefore, I think that whilst this subject, and the difficulties felt by a medical man on approaching it, are still fresh in my mind, a short historical and explanatory article, drawn from the study of the under-mentioned* works may be acceptable to medical readers. I have found, however, that the treatment of the subject has expanded greatly beyond my original intentions, and has taken rather the form of a treatise on the relation of physics in general, and more especially of the doctrine in question, to physiology, in which the attempt will be made to show that the doctrines of Fletcher respecting the nature of life must occupy a most important position if we are to bring the facts and laws of physiology into harmony with those of physics and chemistry. For, while on the one hand he was a consistent opponent of the doctrine which attributes vitality to the addition of any immaterial essence or entity to ordinary matter, such as the so-called vital principle, yet, on the other hand, he was a strict and consistent vitalist, denying that any merely chemical change ever did or could take place in the living matter properly so called. On his principles it will also, I hope, be made plain that life cannot be said to consist in any force, and that, in fact, the expression "the vital force" which we so often hear is erroneous and even incongruous.

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* *Die Mechanik der Wärme*, by J. R. Mayer. Stuttgardt, 1867. (Contains a collection of all the works of this author.) *Abhandlungen uber die Mechanische Wärmetheorie*, von R. Clausius. Two vols., 1864 to 1867. Arnott's *Physics*, 1864. Miller's *Chemical Physics*, 4th ed. Odling's *Manual of Chemistry*. Tyndall on *Heat*, 3rd edit. *Phil. Mag.*, 1840 to 1870, containing papers by W. Thomson, Joule, Rankine, Regnault, Helmholtz, Faraday, Waterston, Tait, Verdet, Colding, Akin, Bohn, Heaton, Maxwell, Challis, &c. Carpenter on "The Correlation of Physical and Vital Forces," *Quart. Journ. of Science*, 1864. Bence Jones, *Croonian Lectures on Matter and Force*. Williamson on *The Atomic Theory*. C. Brooke, *Natural Philosophy*, 1867. Fownes's *Chemistry*, 10th ed., 1868. Fick, *Die Naturkräfte*, 1869. "Renaissance de la Physique Cartesienne," par Bertrand, *Journal des Savants*, 1869 "L'esprit de la Physique Moderne," par A. Laugel, *Rev. des deux Mondes*, Sept., 1858. *Sketch of Thermodynamics*, by Professor Tait, 1868. *Ganot's Physics*, by Atkinson, 1868. Orme, *On Heat*. Helmholtz's "Lectures," *Med. Times*, 1864, &c., &c.

In illustration of the present transition-state of physical science in respect to the doctrine of the conservation of force, let me adduce the following quotation from the last edition of Arnott's 'Physics,' in which is inserted a short statement of the new doctrine, while, at the same time, the old theories of familiar phenomena have not been remodelled in accordance with it.

"The latent heat of common air is made sensible in the *match syringe.* In this, which is a tube closed at the bottom, the piston is driven down quickly and strongly, so as to compress very much the air which is underneath it, and the heat then condensed with the air is sufficiently intense to light a small piece of tinder attached to the bottom of the piston."—*Arnott's Physics,* 1864, part i, p. 21.

Here the heat is supposed to pre-exist in the air, and simply to be given out owing to the change of capacity induced by the compression of the air. The new doctrine is very different, for according to it the heat is newly generated, and represents the exact amount of the force used by the arm in compressing the air. This force is not destroyed as was formerly supposed, in overcoming the resistance, but undergoes transformation into heat. But the arm itself derives its force from an exactly equivalent amount of vito-chemical change in the nerves, muscles, and blood, involving a corresponding consumption of these materials. The store of force-yielding blood must be replenished from the food; that again is all derived mediately or immediately from plants. These, again, derive their whole store of force which enables them to build up the complicated organic products serving as food for animals, from the heat and light of the sun.

Whence then the heat of the sun, and how is it maintained? The new doctrine is competent to answer this with a high degree of probability. By the splendid hypothesis of Mayer, engrafted on that of La Place, the contraction of the nebular mass of the matter of our solar system is exhibited as a sufficient cause of the original heat of the sun. The fall of this vast mass of matter through such immense distances is competent, according to the calculation of Helmholtz, to generate in the sun a heat of 22,000,000 degrees, Cent., the far greater portion of which must have been dissipated long before the earth assumed its present state. For it is also a part of the doctrine in question that all forms of force or energy are mutually convertible, but, practically, the final result of all such transformations is into heat or light. These, as is well known, are perpetually radiating into the surrounding medium; and hence, with the exception of such portions as are absorbed by plants and chemical action, the tendency is finally to be dissipated into space. This dissipation of the sun's energy is perpetually going on at an enormous rate, of which we can form some conception by a knowledge of the fact that the total amount of it intercepted by the earth is equal to only the 2,300,000,000th part of the whole.

How, then, is it renewed? The theory of Mayer is competent to explain this also; for it has been shown that the continual falling into the sun of the masses of asteroids, and other meteoric matter known to be circulating in our solar system, would be sufficient to generate an adequate amount of heat which it has been shown no chemical process such as combustion could possibly do. Ultimately, however, a time must arrive when, by the action of the natural forces resulting in the continual dissipation of energy, and the exhaustion of the meteoric matter, the final extinction of life, heat, and light in our system must ensue, realising the vision of the poet—

> "The bright sun was extinguished, and the stars
> Did wander darkling in the eternal space."

Even here speculation is not arrested, for theories have been formed as to the possible restoration of this lost energy. The sun, then a cold, dark body, moving, as now, through space at the rate of 411,000 miles a day, may encounter a similar moving body, when the force of the collision will generate an amount of heat sufficient to dissipate the mass into the nebular state. Or, again, by the daring and beautiful hypothesis of Rankine, it is supposed that the interstellar æther does not fill infinite space, but beyond its boundaries there is an infinite void. When the waves of radiant heat reach the boundary of the æther they will be totally reflected, and ultimately reconcentrated into foci. "At each of these foci the intensity of the heat may be expected to be such that should a star (being at that period an extinct mass of inert compounds) in the course of its motions arrive at that part of space, it will be vaporised and resolved into its elements; a store of chemical power being thus reproduced at the expense of a corresponding amount of radiant heat."* (*Ph. Mag.*, IV, vol. iv, p. 360.) Thus, or in some such way, may the cycle of changes of a sun and planetary system be again begun, and so on for ever and ever, as far as science can teach us, for to her matter and force are eternal and indestructible, and of a beginning or an end she can tell us nothing.

Such is the programme of but a small portion of the rôle to be played in science by the grand doctrine of the conservation and equivalence of force. The whole turns on the *discovery* of what is termed the *mechanical equivalent of heat.* To comprehend the significance of this statement let us revert to the state of mechanical science during last century. A rapid advance then took place in the invention of machinery, and during that time a number of ingenious persons turned their minds in this direction before the fundamental principles of mechanics were fully understood; and seeing the practical gain of power from the use of the lever, screw, pulley, &c., they hoped by some clever arrangements to effect a real gain of driving force, or, in other words, obtain "perpetual motion." As alchemy was the parent of chemistry, this search, though futile in its effect in itself, led to the discovery of the great principle now under consideration. For, ere long, by study and experiment, the true principles of mechanics were evolved, and the laws which govern them were stated with precision. On this point, after giving a variety of illustrations of mechanical equivalence from machinery driven by various powers, Helmholtz* says:—"From these examples you observe, and the mathematical theory has corroborated this for all purely mechanical, that is to say, for moving forces, that all our machinery and apparatus generate no force, but simply yield up the power communicated to them by natural forces—falling water, moving wind, or by the muscles of men and animals. After this law had been established by the great mathematicians of the last century, a perpetual motion, which should only make use of pure mechanical forces such as gravity, elasticity, pressure of liquids and gases, could only be sought after by bewildered and ill-instructed people."

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* Clausius (vol. i, p. 323) has discussed this hypothesis in a profound mathematical article, and pronounces it untenable according to the laws of heat. But the human mind craves for an answer to the question, Is the energy of the universe being perpetually dissipated in the ever-diminishing wavelets of heat and light for ever and ever? or has the interstellar æther an end, beyond which nothing but infinite void exists? In which case what becomes of the force of the undulations of heat and light? Can it be annihilated there any more than here; if not, must it not be reflected in some form of force, and so restored to the material universe? Has no means been devised to "gather up the fragments that remain, that nothing be lost" in the housekeeping of the Almighty?

But the question was still undetermined with respect to the other natural forces—heat, electricity, magnetism, and chemical action. Did the same law apply here? and what was the real relation between them and mechanical motion, which they can all produce? Might there not still be possible some ingenious combination of these forces from which a real gain of power might be got? for example, by turning the electro-magnetic machine we obtain a current of electricity; by that we might decompose water; and by collecting the resulting oxygen and hydrogen might we not by their combustion obtain a flame which, if directed on a piece of lime, would produce the well-known Drummond light? at the same time great heat is given out, and might not that be used to boil the water and thus drive a steam-engine sufficiently powerful to turn the magnet? In this way should we not obtain a splendid light by a self-acting machine? There are many persons, even among those who boast of the enlightenment of our age, who could not answer off-hand that such a result was impossible, and there are not many persons who could show why it is so. When we pass to the more complex and (as yet, in many respects) mysterious phenomena of the organic world we find that, even among physiologists, there are still many who are not prepared to admit that no amount of force of any kind, even to the turning of a hair, can come out of any animal body beyond the exact amount that went in, as food, air, and heat.

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* "On the Interaction of Natural Forces." (*Phil. Mag.*, IV, vol. xi, p. 495.)

The question of the possible gain of power by the interaction of the so-called "imponderable forces," was brought to an issue by the repeated efforts of inventors to obtain the greatest possible power from the application of the principle of the steam engine. The chief, if not the only, practical way in which mechanical work can be got by heat is through the expansion of gases and vapours; and as the rate of expansion of all these is nearly the same, while the boiling point of liquids varies greatly, might not these differences be taken advantage of for the actual gain of motive power? If not, why not? and what is the true law which governs the proportion between work done and fuel expended in a heat engine? Singularly enough this was discovered before its explanation could be given, just as the practical limits of the power of the syphon and the pump were known before the true nature of suction was discovered, and it was referred to the supposed "horror of a vacuum" existing in nature. This discovery was announced in an essay by Sadi Carnot, published in 1824, entitled *Reflexions sur la Puissance Motrice du Feu.* According to Reguault this work attracted little attention at the time in France, but more recently its importance was recognised by W. Thomson and R. Clausius, who confirmed, with some modifications, a law therein laid down by Carnot, and since called by his name. The substance of this law is shortly this—that it is only when heat passes from a warmer to a colder body that work can be done, that this is independent of the nature of the working substance, and is in proportion to the difference of the two temperatures.* What is chiefly interesting to us at present is to note the manner in which Carnot attempts to explain how work is produced from heat. We must remember that at that time heat was supposed to be an imponderable material substance called caloric, which could be contained in greater or less quantity in any body, and could be transferred from one body to another, and could even exist in two very different states described as latent and free; but that the total quantity of it existing in the world could never be increased or diminished, because no kind of matter could either be created or annihilated. With these views of heat how could its work-producing powers be explained? Carnot says—

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* Carnot's law :—"The ratio of the maximum mechanical effect to the whole

> "If a body after having experienced a certain number of transformations be brought identically to its primitive physical state, as to density, temperature, and molecular constitution, it must contain the same quantity of heat as that which it initially possessed; or, in other words, the quantities of heat lost by the body under one set of operations are precisely compensated by those which are absorbed in the others. This fact has never been doubted; it has at first been admitted without reflection, and afterwards verified in many cases by calorimetrical experiments. To deny it would be to overturn the whole theory of heat in which it is the fundamental principle."

I have not seen his work myself, but we have it on the authority of Regnault and Clausius that Carnot really believed the entire quantity of heat came out undiminished. This we know now to be false, and the experiments on which he relied to be incorrect; but supposing it to be true that the whole heat came out undiminished, and that work done depended solely on the passage of heat through the medium from a warmer to a colder body, how then does he account for the motive power? He can find no better mode of explanation than by comparing the sinking down of a mass

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heat expended in an expansive machine is a function solely of the two temperatures at which heat is respectively received and emitted, and is independent of the nature of the working substance." The same modified by the dynamic theory of heat, and adapted to it by Thomson and Clausius.—"The maximum proportion of heat converted into expansive power by any machine is a function solely of the temperatures at which heat is received and emitted by the working substance, which function for each pair of temperatures is the same for all substances in nature." (From Rankine's paper on "The Mechanical Action of Heat." *Phil. Mag.*, IV, vol. vii, p. 249.)

of caloric to a lower temperature to the similar falling down of a mass of water, and producing motive power in the same way. He uses in fact the term, "une chute de calorique," as a parallel expression of "une chute d'eau." (Clausius I, p. 298.)

Now, it is quite true that, if all bodies in nature had the same temperature, it would be impossible to convert any portion of their heat into mechanical work; and also that the heat of a body, which we are unable to cool further, cannot be changed into another form of force. (Helmholtz.)

It has also been erected into a principle by Clausius (I, p. 134) that "heat can never of itself pass from a colder to a warmer body," and, therefore, a compensating equivalent of force must be expended—not gained—if a body is cooled below the surrounding media. On the other hand, also, though heat by its nature is perpetually striving to pass to colder bodies by conduction and radiation, and although every doing of work by it is accompanied by, and proportionate to, the fall from one temperature to another, nevertheless, as W. Thomson observes, the converse holds good, and it may very well happen, as, in fact, it does happen, that heat may pass from a warmer to a colder body without doing any work, as is the case in countless instances of simple conduction and radiation. Therefore Carnot's hypothesis affords no explanation of the cause of heat doing work; and the fact that nothing better could be offered by a man of such talent affords an excellent illustration of the obscurity in which the question was enveloped.

From this time for about sixteen years no further advance took place in the theory of the subject, and though the true nature of heat had virtually been discovered by Rumford and Davy long before, it still failed to receive acceptance. Gradually, however, it dawned upon the minds of scientific inquirers that the great desideratum was an exact numerical relation between the imponderable forces. Accordingly, between the years 1839 and 1843, several men of science were working in this direction simultaneously and quite independently of each other. Of these we may here mention Séguin, and Colding, merely for the sake of historical accuracy; for though by speculation and experiment they made an approach towards the true conception of the subject, yet they cannot be credited with having fully apprehended the law of equivalence of force, or having contributed to its final establishment. The glory of this discovery belongs to Julius Robert Mayer, of Heilbronn, and James Prescott Joule, of Manchester, who arrived at it simultaneously and independently of each other. As is usually the case, the question of disputed priority has been raised, but, as I think will be shown, without reason, and the German and Anglo-Saxon branches of the Teutonic race may rejoice equally in being able to count two such men among their sons. It may be safely predicted that future generations will look back upon them in the same way that we regard Galileo, for the importance of this discovery entitles them to rank second only to Newton and Copernicus.

Apart from the intrinsic interest of the narrative we shall probably understand the subject best by following the footsteps of the discoverers. We shall commence with Mayer, who had the advantage of a slight priority of publication, and who has an additional claim upon our interest, inasmuch as he belongs to our own profession. It is remarkable that the first intimation of this discovery should have come not from the physicists, with all their apparatus and opportunities, but from one who, like ourselves, was placed in the most unfavorable circumstances for continuous experiment and research.

Before giving an abstract of the original paper enunciating the discovery, it will be well to give a *résumé* of the train of thought which led him to it, as contained in a later publication.

In the preface to his explanatory paper he says, "Why one occupied like myself as a physician in practice should have ventured to put forth my word in this important question is sufficiently stated in the work itself. May competent judges who know the difficulties with which the cultivation of a new field in science is accomplished extend a lenient judgment to my labours! *Ars longa, vita brevis.*" Dated, "Heilbronn, in the month of Christ, 1850." These words at once bespeak our sympathy.

§ 1. In an introductory chapter he now proceeds to explain wherein the *à priori* method of the ancients, and especially of Aristotle, failed to discover the true relations of natural phenomena; and also to insist upon the necessity of experiment and exact numerical relation in all scientific research.

He tells the history of the discovery as follows:—In the year 1840 he was surgeon of a ship, carrying a crew of twenty-eight. The voyage to Batavia lasted a hundred days, during which time the crew remained in good health. Whilst at sea the temperature was on the whole cool, and varied within a range of 15° Cent. in the twenty-four hours. But on their arrival at Batavia the average variation was scarcely 5°, and the temperature was high and the air sultry. An acute inflammatory affection of the lungs now broke out among the crew. He bled copiously, and was struck by the fact that the blood from the vein of the arm *had such a bright colour that he feared he had punctured an artery.* This was only observed in newly-arrived Europeans, for after acclimatisation the blood resumed its ordinary dark colour in the veins.

> "This phenomenon riveted my full attention. Setting out from Lavoisier's theory that animal heat is the result of a process of combustion, I regarded the double change of colour, which the blood undergoes in the capillaries of the greater and lesser circulation, as a sign perceptible to the senses, a visible reflection of the oxidation going on in the blood. To maintain an equable temperature in the human body, the extrication of heat within it must stand in a proportional relation to the loss of heat from it, and thus also with the temperature of the surrounding medium; therefore the production of heat, and the oxidative process, and also the *difference of colour* between *arterial and venous blood,* must, on an average, be smaller in the tropical than in the colder zones." (page 250.)

He also explains further, in another place, that the slightness of the difference of colour arises from the fact that the blood-corpuscles still continue to take up in the lungs the quantity of oxygen they were accustomed to do in the colder regions, which not being now required for the production of heat is not consumed in the systemic capillaries, and hence the bright colour of the venous blood.* But this disproportion, as above said, lasts only a short time.

A great portion of the blood being devoted to the production of heat it becomes of importance to know exactly the balance between the consumption of material and the heat obtained. Now, there are two ways in which heat can be developed: 1st, that in which animals produce it directly; and, 2nd, that in which it is produced indirectly through muscular action by way of friction and the like, even at a distance. It, therefore, became of importance to know "whether the directly developed heat alone, or whether the sum of both the directly and indirectly produced heat is to be put to the credit of the oxidative process," p. 251. This question goes to the root of the matter, and must be answered quantitatively.

The physiological combustion theory implies that the amount of heat evolved from any given quantity of matter burned is *invariable* and independent of any accessory circumstances. So no possible influence of the living organism can augment it, or, in fact, make heat out of nothing. The sum of the heat can, therefore, never be greater than that allowed by the chemical action; "hence the total heat developed in the organism whether directly or whether mediately through mechanical motions must correspond quantitatively, or be equal to the oxidative changes. Thence follows also, by the same necessity, *that the heat generated by a living body through mechanical processes must stand in an immutable quantitative relation to the work expended thereon;* and from this, again, it follows that an immutable quantitative relation between heat and work is a postulate of the physiological combustion theory of heat," p. 253. He was thus pushed on to investigate the general question of the relation between heat and work, and was convinced that the mechanical equivalent of heat must soon be discovered, and ere long he himself fell upon a mode of determining it.

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* This observation has been singularly neglected by pathologists. Quite recently, however, we have a most important observation by Dr. Richardson on the effect of the weather on the mortality of capital operations in surgery, which seems to have a bearing on the subject. He attributes the surgical fever and death to abnormal increment of heat caused by the impairment of those conditions which produce equalisation of temperature through evaporation or other means, while the production of heat by oxidation still goes on at the former rate. (*Med. Times*, Jan. 29, 1870.)

"In order to make my discovery secure against eventualities, I put together the essential particulars in a short article, which I sent to Liebig in the spring of 1842, with the request to have it published in the *Annalen der Chemie und Pharmacie*, where, in fact, it was published in vol. xlii, p. 233 (May, 1842), under the title "Bemerkungen über die Kräfte der unbelebten Natur.''

As this paper, besides its intrinsic merits, is of historical interest, I give here an abstract of it.

§ 2. He begins with the proposition "Forces are causes, and in respect to them, the axiom *causa æquat effectum* finds full application.'' In a chain of causes and effects no link can be wanting, whence the first property of all causes is their indestructibility. If one link, say *c*, has produced an effect equal to itself *e*, it ceases to exist as *c*, and so on through the whole chain. Therefore, all these quantitatively equal links must be regarded as different forms of the same object. "The power of taking on different forms is the second essential property of all causes. Both properties being comprehended, we say causes are (quantitatively) indestructible, and (qualitatively) convertible objects.'' But there are two divisions of causes in nature, between which no transference can occur, viz. those possessing impenetrability and weight, *i. e.* material, and those without, *i.e.*, the imponderable forces. "Forces are therefore *indestructible, convertible, imponderable objects*.''

"A cause which brings about the raising of a weight is a force; its effect (the raised weight) is, accordingly, equally a force; or, expressing this relation in a more general form, *separation in space of ponderable objects is a force;* since this force causes the fall of bodies, we call it *falling force*. Falling force and fall, or, more generally still, falling force and motion, are forces which are related to each other as cause and effect—forces which are convertible one into the other—two different forms of one and the same object. For example, a weight resting on the ground is not a force; it is neither the cause of motion, nor of the lifting of another weight; it becomes so, however, in proportion as it is raised above the ground: the cause—the distance between a weight and the earth—and the effect—the quantity of motion produced—bear to each other, as we learn from mechanics, a constant relation. Weight being regarded as the cause of the falling of bodies, gravitating force is spoken of, and so the notions of *property* and of *force* are confounded with each other; precisely that which is the essential attribute of every force—the *union* of indestructibility with convertibility—is wanting in every property: between a property and a force, between weight and motion, it is therefore impossible to establish the equation required for a rightly conceived causal relation. If weight be called a force, a cause is supposed which produces effects without itself diminishing, and incorrect conceptions of the causal connection of things are thereby fostered. In order that a body may fall, it is no less necessary that it should be lifted up than that it should be heavy, or possess weight; the fall of bodies ought not, therefore, to be ascribed to their gravity alone."

We see in numberless instances a motion cease without causing another motion or raising a weight, and as a force once existing cannot be annihilated, but must merely be changed into another form, the question is, therefore, what other form can the force of falling bodies or ordinary motion put on? Experiment alone can tell us this, but to try it properly we must choose materials which, while they destroy motion, will themselves undergo the least possible change. If we try the experiment by friction of metallic plates on each other, we find, as movement is arrested, *heat* is developed. Is then the motion the cause of the heat? "In order to attain to certainty respecting this relation, we must sift the question, whether in the numberless cases in which by expenditure of motion heat appears, the motion may not have another effect than the production of heat, and the heat another cause than motion?"

When we regard the enormous quantity of motion that may be brought to a stand by the friction of two metal plates, could any one expect to find in the abraded particles even a trace of the vanished force, and reduce it from them? Nevertheless, the motion could not have been annihilated any more than it could have originated from nothing. In short, without the recognition of the causal relation between heat and motion all explanations fail. No mere change of aggregation, nor of the capacity for heat, will suffice for explanation; nor will the mere change of volume. "As is well known, you may melt two pieces of ice by friction *in vacuo*, but will any amount of simple pressure convert ice into water?"*

"The author found that by strong agitation the temperature of water could be raised. The water thus heated (12° and 13°

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* A negative reply is not absolutely correct, for dead pressure on ice at or near the freezing point will produce melting. But this is no exception to the general laws of force, for pressure on water lowers the freezing point by counteracting the expansion which takes place on freezing. Also water contracts on being heated from 0° to 4° Cent., and it was found by Joule that a pressure of twenty-four atmospheres causes between those temperatures a certain amount of cooling.

Cent.) occupies a greater volume after the agitation than before; now, whence comes this amount of heat which may be developed by repeated agitation in the same apparatus as often as we please?"* He then goes on adducing other examples, showing by the disappearance of heat and arising of motion the apparently causal relation between them, and among other remarks has this: "The locomotive with its train may be likened to a distilling apparatus; the heat under the boiler is converted into motion, and that again is given back in the axles and wheels as heat.

"We bring to a close our theses—which are necessary consequences of the fundamental principle *causa æquat effectum*, and are in complete harmony with all the phenomena of nature—with a practical conclusion. For the solution of the relations between the force of gravity (fallkraft) and motion, the distance fallen through in a given time, *e. g.* the first second, must be determined by experiment; just in the same way must the relation of heat to motion be determined quantitatively. For example, we must find out how high above the ground a certain weight must be raised, that its falling force should be equivalent to the warming of an equal weight of water from 0° to 1° Cent. That such an equation must actually exist in nature may be regarded as the *résumé* of the foregoing. By application of the foregoing principles to the relations of temperature and volume of the gases, we find the sinking of a column of mercury, which compresses a gas, to be equal to the heat developed by the compression, and from this it follows—the relative capacities for heat of atmospheric air at constant volume and constant pressure, being taken as 1 to 1·421—that the falling of a certain weight from a height of about 365 metres, must correspond to the heating of an equal weight of water from 0° to 1° Cent." He then concludes with a sentence remarking how small, on this computation, is the work done even by the best steam-engines in comparison to the heat of the furnace.*

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* The paragraph continues thus:—"The hypothesis of thermic vibrations inclines to the position that heat may be the effect of motion; it does not, however, estimate this causal relation at its full value, but lays the chief weight on inharmonious (unbehagliche) vibrations." He continues the argument that if no other effect can be found for extinguished motion than heat, and no other cause for the heat than motion, we may conclude their causal connection, rather than suppose an effect without cause, and *vice versâ*. He then notices the natural connection between the force of gravity, motion, and heat, and the necessarily proportional relation among them, and then says, "[Nevertheless, as little as it can be deduced from the connection between gravity (fallkraft) and motion, that the essential nature of falling-force is motion, just so little does this conclusion hold for heat.] We might rather draw the opposite conclusion, that to be able to become heat the motion—be it a simple or a vibrating one, like light, radiant heat, &c.—must cease to be motion" (p. 10.) Professor Tait quotes this passage, omitting the part above in brackets, and makes this remark—"Mayer does not accept the conclusions deduced by Davy and Rumford from their experiments and now almost universally followed." It is abundantly evident in the sequel that Mayer does accept the vibratory theory of heat; but that is not the point under discussion here. For it is well known that Davy and Rumford failed themselves, and the whole scientific world failed with them, to see the true nature of the imponderable forces as a whole till the exact numerical relation of heat and motion was pointed out by Mayer and Joule. That is the point here insisted on, and it is to bar any chance of Davy's theory disproving the equivalence that the above remarks tend. In truth Mayer takes the comprehensive view of force from the beginning which is now done under the term energy; and he rather avoids any speculation as to the exact nature of heat, in order to keep the argument clear. The cause of gravity is not known (attraction between the particles of matter being a mere mathematical assumption), but the motion of a falling body comes within the law of equivalence of force. Pray, why did Mr. Tait leave out the first part of the sentence above? Does he believe that the cause of gravity is a vibration? This question is discussed by Mayer at pp. 279—282, and such criticisms as the above are answered by anticipation twenty years ago.

§ 3. Such is the now celebrated original essay of Mayer. It is very short, occupying barely twelve pages. As might have been expected from the preliminary train of thought we have indicated, it approaches the subject from fundamental philosophical principles. It first distinguishes between the inherent *properties* and the *forces* of matter, defining the latter as indestructible, equivalent, and convertible objective existences, the proof of this being made to hang on the exact numerical relation between heat and movement. Speculation on the nature of heat is avoided, and all stress is laid on the general doctrine of the equivalence and conservation of force in all its forms. From its condensed and apparently fragmentary character, which was partly forced upon him by the difficulty of obtaining space for publication,† its importance was not discerned at the time. This was, perhaps, fortunate for his own fame, as it gave him the opportunity of working out the subject alone, and showing, by his subsequent essays, that he had apprehended the full scope of this grand doctrine from the beginning. Had he given the full calculation the result might have been different. He has done this in his later publications, and I think we cannot do better than follow him in studying this one point exhaustively.

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* A translation of this paper is contained in the *Phil. Mag.*, IV, vol. xxiv, p. 371.

† Mayer speaks with gratitude and respect of Liebig, and gives him the credit of being one of those whose minds were working in the same direction as he, and that he referred the heat produced mechanically by a steam engine solely to the effect of the combustion, and said the latter can never be exceeded by heat produced by the work of the engine. We cannot allow, however, that Liebig clearly apprehended Mayer's doctrine in his *Animal Chemistry* in 1846, and even in Gregory's *Turner's Chemistry*, 1847, to which Liebig's name is attached, the

§ 4. There were two practicable modes of solving this problem,—1st, To find the quantity of heat consumed, *i. e.* put out of existence, when work is done by a medium, such as a gas expanding; and 2nd, the quantity of heat generated by the expenditure of work in friction. To apply the first method to such a complicated machine as the steam engine involved insuperable difficulties, so he turned at once to the more definitely known ratio between temperature and the expansion of gases. It has long been known that if a certain quantity of air be heated at constant volume, *i. e.*, in a vessel strong enough to prevent its expansion, or, in other words, under a sufficiently *increased pressure* from without, it will require a certain quantity of heat to raise its temperature to a given degree. But if the same quantity of

remarks on heat of chemical combinations (p. 185) show that the doctrine had found no entrance into his mind. We are told by Helmholtz that Mayer was at first repulsed by the physicists, and could with difficulty obtain room for his first condensed paper. Afterwards his subsequent works were published in the form of separate essays, and long overlooked amid the flood of books constantly poured forth. In consequence of his treatment and the pressure of his mental labour, soon after 1851 "his overtasked brain gave way, and a cloud settled over the intellect which had accomplished so much," as is expressed by Tyndall. For a while he was secluded from the world, but happily his health was again restored, and he has come forward to take his proper rank in the scientific world. Though he does probably owe the acceptance of his first paper to Liebig, still we see by subsequent events how grudgingly it was accorded, and most likely a longer paper would have been rejected. We can fancy the supercilious tone of the physicists and chemists towards him at first. "What could a country doctor have to tell us about such matters?" The tables are now turned, and the country doctor's name will go down to posterity through all time, while the editors of the leading journals of the day, if their names survive at all, will be to him as hewers of wood and drawers of water.

air be heated in a yielding bag or under a movable piston, so that it is allowed to expand against the pressure of the atmosphere, *i. e.*, at *constant pressure,* it will require a known greater amount of heat.

§ 5. The ratio between the heat at constant volume to that at constant pressure was given by Dulong and Petit, as 1 to 1·421. That is to say, if it requires 10 grains of fuel to raise the temperature 1° in the former case, it would take 14·21 in the latter. What is the cause of this remarkable phenomenon? It was formerly almost universally attributed to the increased capacity for heat possessed by the more rarefied air. By Mayer, on the other hand, the surplus heat was held to represent that consumed in doing the work of raising the piston, or of expanding the bag against the weight of the atmosphere. The actual calculation, which was published in the '*Organische Bewegung,*' p. 29, may now be given.

§ 6. "The cubic centimetre of air at 0° Cent. and barometer 0′·76, weighs 0·0013 grains; at constant pressure heated 1° C., it expands $\frac{1}{274}$th of its volume, and thus raises a column of Mercury of one square centimetre surface and 76 centimetres high by the degree of $\frac{1}{274}$th of a centimetre. The weight of this column amounts to 1033 grains. The specific heat of air at constant pressure is, according to Delaroche, and Berard, 0·267, water being = 1; the amount of heat which our cubic centimetre of air takes up in order to be raised from 0° to 1° at constant pressure is thus equal to the heat by which 0·0013 × 0·267 or 0·000347 of a gramme of water may be raised 1°. According to Dulong the amount of heat which the air takes up at constant volume is to that at constant pressure as 1 to 1·421; hence the amount of heat which our cubic centimetre of air at constant volume takes to raise it 1° is

$$\frac{0{\cdot}000347}{1{\cdot}421} = 0{\cdot}000244 \text{ degrees.}$$

Therefore the difference between the two, *i. e.* 0·000347—0·000244 = 0·000103, is the degree of heat by the consumption of which the weight, 1033 grammes, was raised $\frac{1}{274}$th of a centimetre. By reduction of these figures we find that 1 gramme of water heated 1° Cent., or

$$\text{One unit of heat} = 1 \text{ gramme raised} \left\{ \begin{array}{l} 367 \text{ metres.} \\ 1130 \text{ Paris feet.''} \end{array} \right.$$

(page 30.)

This, again, reduced to English feet and pounds, and Fahrenheit's scale, gives 668·6 as the equivalent of heat. Now, the average of Joule's first thirteen experiments, in 1843, gives 838, and the individual figures vary from 587 to 1040. Consequently, Mayer's original one was almost as near the true number as Joule's. But this does not give a correct idea of the completeness of Mayer's method, for the data on which he calculated, though furnished by the highest authorities at the time, have been corrected. · No one can possibly revise all the data he must use; there must be division of labour in the vast field of natural science, so we can only judge the method by re-calculating by the more recent data. I give, therefore, the formula of Clausius, who repeated Mayer's estimate on a slightly different plan in 1850, and got the figures 370. But in 1864 he adds a note requiring the following alterations. The co-efficient* of expansion of gases is made $\frac{1}{273}$. The specific heat after Regnault is 0·2375: the ratio of specific heat at constant volume to constant pressure is 1 to 1·41.† I give here the formula re-calculated according to these changes.

§ 7. The pressure of the atmosphere (·760 m.) on a square metre amounts to 10333 kilogrammes; the volume of 1 kilogramme of atmospheric air under the above pressure, and at 0· Cent. is equal to 0.7733 cubic metre. Hence

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* The coefficient of expansion of gases means the number by which is expressed the amount of expansion from one degree of heat. This has been found to be equal at all temperatures and for all the permanent gases—a law which is usually attributed to Gay Lussac, though Professor Tait recently says erroneously, its discovery being really due to Charles. The amount was stated by Gay Lussac and Dalton as $\frac{1}{267}$; then by Rudberg at $\frac{1}{274}$, the amount adopted by Mayer; lastly, Magnus and Regnault make it $\frac{1}{273}$, the number now generally adopted, *i. e.* all gases expand $\frac{1}{273}$ of their volume at 0° Cent. for each degree Cent. of heat.

† Clausius, I, p. 76. Rankine, *Phil. Mag.*, IV, 5, p. 137.

$$\frac{10333 \times {\cdot}7733}{273} = 29{\cdot}27.$$

The specific heat of air is now taken as 0·2375, and the ratio at constant volume to that at constant pressure as 1 to 1·41. Therefore,

$$\frac{1{\cdot}41 \times 29{\cdot}27}{{\cdot}41 \times 0{\cdot}2375} = 423{\cdot}8 \text{ kilogrammetres.}$$

In Fahr. and ft. lbs. $\frac{5 \times 423{\cdot}8 \times 3{\cdot}28}{9} = 772{\cdot}24$ ft. lbs.

The same calculation made by Tyndall with air in Fahr. and English foot pounds is	771·4 ft. lbs.
The same repeated by Orme with hydrogen, gave 423·653 kilogrammetres, or	771·98 ft. lbs.
Joule's final result of six years of experiments by the direct method of fluid friction is..................................	772 ft. lbs.

The coincidence of all these numbers not only removes every shadow of doubt as to the relation of work and heat, but also shows the perfection of Mayer's original method.

The mere statement of these results conveys, to all but advanced physicists, a very inadequate impression of the difficulties and importance of the subject. It is, therefore, desirable to follow it out in detail, and as I am writing for the busy practitioner I may call to mind the opinions respecting specific and latent heat which were then current.

§ 8. It is well known that the thermometric temperature of a body affords us no indication of the actual quantity of heat which such a body contains; or, rather, which must be communicated to that body in order to raise its temperature to any given amount, and which it gives out on cooling. All bodies differ in this respect; mercury, for example, requires above thirty times less heat to raise its temperature than water, and as they are all compared with water, as 1, this relation of any body is called its *specific heat*, or its capacity for heat, which latter term was consistent with the material theory of heat. What interests us here is the fact that the same bodies are susceptible of alterations of specific heat under certain circumstances, and it was by means of these alterations that the phenomena under consideration were attempted to be explained.

"A soft iron nail may be made red hot by a few dexterous blows on an anvil, but the experiment cannot be repeated until the metal has been *annealed*, and in that manner restored to its former physical state." (Fownes, p. 61.)

"The specific heat of a piece of soft copper was found to be from 0·09501 to 0·09455; the same copper after hammering had a specific heat of from 0·0936 to 0·0933; on being again thoroughly annealed, so as to recover its former density, its specific heat was from 0·09493 to 0·09479, or almost the same as at first." (Miller, i, p. 303.)

Such facts were thought by Black to be confirmatory of the material theory of heat; and it certainly does seem as if the hammering had forced out the heat from the iron like water from a sponge, and that a fresh supply was taken up when it was put in the fire. Thus the production of heat and cold by friction, compression, and expansion might be held to be explicable by the alterations of specific heat. But the unlimited production of heat by Rumford from the friction of metals overthrew this with respect to solid bodies as a general principle, and the above phenomena must be ascribed to some merely partial molecular disarrangement. In spite of Rumford's discoveries the material theory, as we know, still held its sway, and continued to be applied in explanation of the relation of heat to the expansion and compression of gases. For example, if compressed air be allowed to expand suddenly, a perceptible stream of cold is produced; and liquefied *Carbonic acid* so treated produces cold so intense that portions of the acid are frozen into flakes like snow. Likewise the first strokes of the air-pump produce a cloud in the receiver from the chilling of the expanding air. On the other hand, sudden compression of air is attended with the development of heat, as in the "match syringe" already alluded to. The alterations of specific heat were still supposed to be the cause of these phenomena. It was thought that the rarefied air required combination with a greater quantity of heat for the greater repulsion of its particles, and *vice versâ* with respect to compressed air. The so-called "latent heat" of fluidity and vaporization was explained on much the same principles. In proof of the above we may quote a very remarkable statement of Faraday given by Tyndall.

§ 9. "I have recently found a case mentioned by Faraday, *Researches in Chemistry and Physics*, p. 221, where the effect referred to in the text was, in substance, observed. Faraday's explanation of the effect is a most instructive instance of the application of the material theory of heat. The observation was made at the Portable Gas Works, in 1827. 'It frequently happens, writes Faraday, 'that gas previously at the pressure of thirty atmospheres, is suddenly allowed to enter these long gas-vessels (cylinders), at which time a curious effect is observed. That end of the cylinder at which the gas enters becomes very much cooled, whilst, on the contrary, the other end acquires a considerable rise of temperature. *The effect is produced by change of capacity in the gas;* for as it enters the vessel, from the parts in which it was previously confined, at a pressure of thirty atmospheres, it suddenly expands, has its capacity for heat increased, it falls in temperature, and consequently cools that part of the vessel with which it first comes in contact. But the part which has thus taken heat from the vessel being thrust forward to the further extremity of the cylinder by the successive portions which enter is there compressed by them, *has its capacity diminished*, and now gives out that heat, or a part of it, which it had a moment before absorbed.' I have italicised the phrases which express the old notion. The difference in capacity here assumed is now known to have no existence."

The double effect of the production of cold and heat in the portable gas cylinder is also noticed by Pouillet (*Elemens de Physique*, 1832, vol. i, p. 421), who also explains it as follows :—"These two phenomena of production and absorption of heat are explained in a satisfactory manner by the changes of capacity which accompany the changes of density in gases."

§ 10. These statements, be it remembered, were made in 1827 and 1832, and are not to be wondered at, seeing that they expressed the current belief of the day in spite of Rumford and Davy. But it is truly astonishing to find the very same doctrine taught in the three chief educational manuals of our students published within the last year or two. We have already mentioned Arnott's explanation of the match syringe; and in the tenth edition of Fownes's *Chemistry*, edited by B. Jones and Watts, we find—

> "It has been already said that when a gas expands, heat becomes latent. If a gas on expanding be not supplied with the requisite heat, its temperature falls on account of its own free heat becoming latent. On the other hand, if a gas be compressed, this latent heat becomes free and causes an elevation of temperature, which, under favorable circumstances, may be raised to ignition. Syringes by which tinder is kindled are constructed on this principle." (Fownes, p. 58.)
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> Again, in Brooke's edition of *Golding Bird*, 1867, we find, "if four or five atmospheres of moist air be condensed into a strong vessel, and allowed to cool down to 50° or 60°, on allowing a small escaping current to impinge on a bad conductor, a small particle of ice will be deposited; this arises from the increased capacity of the expanding air for heat, whereby it is able to deprive the accompanying vapour of its heat."

In the very opposite page Mr. Brooke gives Regnault's law, that the specific heat of gases does not vary with their temperature or pressure. I can only explain these discrepancies on the ground that, as these are all new editions of old works, the former phraseology has been inadvertently retained. We can easily conceive how bewildering such discrepancies must be to a student of the present day.

§ 11. The subject itself is, in fact, very intricate, as in almost all examples of the kind we are familiar with, the true cause of cold by expansion and heat by compression is present as well as the apparent one; and we can with difficulty divest our minds of the idea that the rarefaction of air, *per se*, is a cause of cold, and *vice versâ*. Accordingly, this is the current notion. Likewise the use of the old terms, "latent heat" and "capacity for heat," tends to throw confusion into all treatment of the subject. These difficulties will be explained by the dynamic theory of heat further on; but, in the mean time, we can easily perceive how they must have pressed upon Mayer, in 1841, when, in fact, the above views represented the opinion of the whole scientific world, and we may appreciate the magnitude of the discovery achieved by the young surgeon, just returned from a tropical voyage and in the first year of his settlement in practice. How did he divine the truth in opposition to the voice of the great authorities of the day? Hear him.

§ 12. "When heat is applied to a gas under constant pressure, the free heat of the gas is increased, and at the same time a calculable quantity of heat becomes *latent*, thereby the gas expands, and by this means a quantity of *vis viva* is generated proportional to the pressure and the space passed through. Now, as soon as we can find out how much of the heat which becomes latent is to be put to the credit of the expansion of the gas, then we know also the remainder of the latent heat which corresponds to the evolved *vis viva*. Now it was demonstrated by Gay Lussac by experiment, that the specific heat of a gas which streams out of a receiver into a vacuum undergoes *no* perceptible change. Hence it follows that a gaseous body opposes no perceptible resistance to the separation of its particles, and that by the rarefaction of a gas *per se*, *i. e.* when no evolution of force takes place, no heat becomes latent. Therefore *the whole* of the heat which becomes latent during the expansion of a gas, is the equivalent for the *vis viva* generated. It follows from the principle of the indestructibility of heat, which nobody has ever doubted, that the amount of heat thus become latent must again become free, if by means of the now acquired *vis viva* of motion heat be generated in any manner. Motion is latent heat, and heat is latent motion. A special application of this general principle is the well-known law of Dulong; that the amount of heat which is obtained by the compression of the gases is dependent on the force expended alone, and not upon the chemical nature, nor the temperature, nor the tension of these gases." (page 283.)

§ 13. We thus see that the clue of Ariadne, which guided him through the labyrinth, was the fact of the expansion of gases *in vacuo* without loss of heat. At once, to his mind, this explained the whole mystery; the increase of the *capacity for heat* was evidently a mere figment of the imagination, as here was rarefaction without cold; but, also, *no work* was done in the expansion. Was not, therefore, the work done in expanding a gas under constant pressure the cause of the cold, and not the mere rarefaction? Yet there was nothing new in this experiment of Gay Lussac's: it was known to the scientific world for years.*

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* The remarks of Pouillet on this subject are instructive and interesting as showing the state of scientific opinion respecting it in 1832. After noticing the heating and cooling of the air in the portable gas cylinders, and giving the explanations quoted at § 9, he goes on to say, "M. Gay Lussac has observed on the heat of gases two other phenomena which are more difficult to explain:— 1. The air which escapes from a vessel with a rush through an opening, under

The fact was observed in practice a hundred times a day in the portable gas works, where it was seen, and described, and misinterpreted by Faraday himself. Why, then, did it not give the key to the true theory of force before the time of Mayer? Why, indeed! This is the question asked of all great discoveries. It is true the claim of Mayer to the discovery of the law of conservation of energy has been disputed on the very ground that he did not perform any new experiment * at the outset. But with no reason; for

any given pressure, does not change its temperature, although it expands on issuing from the vessel. 2. A vacuum having been made in a 'ballon,' or the air being merely rarefied, the re-entrance of the external air produces an elevation of several degrees, The first of these facts merits the full attention of the physicists; it appears to indicate that there is heat produced by the rush of the air, and that this heat is the more considerable the greater the difference of pressure which produces the current, so that the reheating compensates exactly the cold produced by expansion. Thus, the second fact is explained by the first." He then states that some physicists thought that the reason why air was warmed by re-entering the vacuum, was that the vacuum itself is full of heat, not merely radiant heat, but an independent and inherent heat which the air appropriates on filling the space. This conjecture was shown to be futile by an experiment of Gay Lussac, in which he suddenly enlarged and diminished the space of a mercurial vacuum, but could observe no corresponding change of temperature. (Pouillet I, p. 422.)

The experiment here spoken of is similar to that alluded to by Mayer, though, in fact, it is only the half of it, as the cold of expansion from a vessel of equal size into the vacuum-vessel is not at the same time noted. The main fact that there was no positive production of cold is, however, made evident, and no proof is given that the heat-production by the friction of the inrushing air could even approximatively balance the cold of expansion then presumed to take place by alteration of specific heat. In other respects, however, Pouillet's explanation of the heat-production by the current of air tallies very well with that now given by Thomson, Joule, and Tyndall. After reading the above one must feel some surprise at the assertion of one of the correspondents in the Mayer controversy (§ 38)—"That it may be gathered from M. Mayer's papers that he knew nothing of this experiment of Gay Lussac's when he wrote in 1842." This is scarcely warranted. Mayer quotes no book in his essay of 1842, but Pouillet's *Elemens* was then one of the best known standard works on physics; it is afterwards frequently quoted by him, and it is to be presumed that he could not fail to have known of these experiments from the first through some such source, and when his interest was aroused to have sought for the original and more complete reports of them.

* Tait at p. 19 says that Colding was led to the idea of conservation of energy by metaphysical reasoning, but that he appealed to experiment, and

were not the experiments already done to his hand by the greatest chemists of the day? The discovery lay in the interpretation of them; and that the merit of this truly

although his results are not to be compared with Joule's, "still Colding evidently went to work in the right way, and deserves an amount of credit to which Séguin and Mayer have no claims." Thus, though he will not name him with Joule, yet he is far above Mayer, owing to *experiments.* Nothing like leather, we see! and we also see how, on occasion, a professor of the so-called exact sciences can be as narrow-minded and prejudiced as his neighbours. As to the mere fact of making experiments, we can see what a really great authority on the subject says in this extract from Helmholtz's letter to Tait himself:—

"The progress of natural science depends upon the constant formation of new inductions from known facts, and the comparison of these inductions, so far as they lead to new consequences, with reality by means of experiment. There can be no doubt of the necessity of this second step; it often requires an extensive application, both of labour and talent, and it brings the greatest credit to him who executes it well. But the glory of the discovery belongs to him who hits upon the new idea; the subsequent experimental verification is often a mere mechanical procedure. And we must not unconditionally require that the discoverer of the idea should necessarily be bound to carry out the second part of the work, for we should thus have to reject the greater part of the work of mathematical physicists. Even W. Thomson published a series of theoretical investigations about Carnot's law and its consequences before he had made a single experiment connected with it, and it would not occur to any of us to value these investigations lightly in consequence."

The subject of Colding's claim possesses an interest in itself, so I may give here the following outline of his views from his own paper in the *Phil. Mag.*, IV, vol. xxvii, p. 56. He sets out with this idea of the relationship of the forces of nature:—"As the forces of nature are spiritual and immaterial entities, whereof we are cognisant only by their mastery over nature, these entities must, of course, be very superior to everything material in the world; and as it is obvious that it is through them only that the wisdom we perceive and admire in nature expresses itself, these powers must evidently be in relationship to the spiritual, immaterial, and intellectual power itself that guides nature in its progress; but, if such is the case, it is consequently quite impossible to conceive of these forces as anything naturally mortal or perishable. Surely, therefore, the forces ought to be regarded as absolutely imperishable." Starting from these principles he performed a number of experiments to be alluded to presently. In 1843 he published his theory and results under the title of *Theses Concerning Force,* wherein he calls attention to the instances already known of heat being disengaged by friction and compression, and points out that these all imply a relation between mechanical energy expended and heat developed, but whether they were in exact proportion had not been ascertained. To determine that he constructed an apparatus and performed a number of experiments amounting to 200. The results he thought belongs to Mayer must be plain to all who have followed his previous train of thought. It was, in fact, because his mind was prepared, and he was looking for the answer that he found it in the above experiments; for ever since he observed the redness of the venous blood in Batavia he had been pondering over the subject, and with a confidence in deductive reasoning from fundamental principles amounting to true genius, he broke through all the obstacles raised by

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were sufficiently accurate to show the principle was right, and he closed the discussion with the argument that, otherwise, the *perpetuum mobile* would be possible. Afterwards he continued his experiments with improved apparatus till 1847, when he published them. In 1856 he wrote an article on the general relationship between the intellectual powers and the forces of nature. This contains cosmical and physical speculations based on the doctrines of the indestructibility of force; it traces the gradual preparation of the earth for the abode of man by the progression of geological, phytological, and zoological changes. Then he supposes a similar progression of the physical forces, and concludes—"not only was it necessary for the ground to be thus prepared and cultivated before human beings could live on the earth, but also it was in like manner necessary that the forces themselves should be prepared and cultivated before they could take on the forces of intellectual life. But, if so, I think it must be satisfactory to us to see that, according to the principles of the perpetuity of energy, we arrive at the conclusion that the intelligent life of man must be intelligent life for ever."

It is plain such unphysical and even mystical speculations cannot be brought into competition with the reasoning of Mayer, who had also the priority; and as regards the discovery of the Mech. Eq. of Heat, Mayer's claims do not rest on personal experiment, but, as we know, on the method of the two specific heats. But when we look into Colding's experiments we perceive his claim runs Joule very close, for we find, as he was an engineer, he constructed an apparatus by which he was enabled to measure the quantity of mechanical power lost, and the quantity of heat disengaged by friction, between brass on the one hand, and brass, zinc, lead, iron, wood, and woollen cloth on the other under different pressures and with different velocities. The result was, that the heat disengaged was always in proportion to the mechanical energy lost; "and if we look more nearly at the figures given in my treatise, which I only presented as a preliminary one, it will be seen that, independently of the materials by which the friction and the heat arose, an amount of mechanical work equal to 359 kilogrammeters should be able to raise the temperature of 1 kilogramme of water 1° Cent., which is very near the proportion (365) that Mayer in 1842 supposed, but did not prove to be right." We cannot wonder that Helmholtz and Verdet give priority to Colding as an experimentalist, for these were published a few months prior to Joule's paper (though unknown to him). But the credit of the actual and final establishment of the discovery by experiment is universally given to Joule.

pre-existing opinions, and discovered in the above well-known facts the true relation between heat and work.

This train of reasoning has already been given in § 2, and may be said to culminate in the axiom, *Causa æquat effectum.** If in mathematics and metaphysics this may be regarded as a mere truism, yet when applied to the physical forces, and conjoined with the axiom that every such cause (*i. e.* force) is expended or put out of existence in the exact

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* On this, Tait, who has taken upon himself the office of Devil's advocate against Mayer, has the following:—

"But when we find, in modern times, conclusions, however ably drawn without experiment from such a text as '*Causa æquat effectum,*' we feel that the writer and his supporters are, as regards method, little in advance of the science of the darker ages. This is one of the fundamental characteristics of all the writings of Mayer, and therefore we may for the present leave them unnoticed, though we shall afterwards have occasion to consider them as furnishing a most admirable development of the consequences of an unwarranted assumption. For, while there can be no doubt that the works of Mayer contain highly original and profound deductions from his premises, deductions of a most important character as regards the system of the universe, it is certain that those premises were unjustified by experiment, and therefore that his method was not merely unphilosophic, but even inconsistent with true science." (p. 47.)

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"Mayer, therefore, and others who have followed a course similar to his, cannot be considered as having any claims to the credit of securely *founding* the science of energy; though their works have become of great value as developments and applications, since the science has been based upon correct reasoning, and rigorous experiments." (p. 48.)

Fully to appreciate the intention of these remarks, the reader ought to know that they come at the end of a dissertation on the errors and absurdities of the methods of the ancient philosophers and of modern metaphysicians such as Hegel, who attempts to solve the problems of physical science by mere speculation. In particular "the extravagant dicta and preposterous nonsense" that were imagined, and gravely taught respecting light, &c., are held up to ridicule. Even the philosophers who spoke of nature abhorring a vacuum, and who searched for the perpetual motion, are extolled in comparison; because they performed experiments, futile and ridiculous though they were. It is among the former class that Mayer is placed in the above quotations, and if the advocate's style [see note to § 37] of writing history is allowable, this method is not devoid of cleverness. Probably few of his readers are acquainted with the writings of Mayer; and there still prevails a good deal of ignorant prejudice, (though happily fast melting away), against the Germans as mystics and muddle-headed dreamers. Having begun the study of this subject with perfect impartiality, and fresh from the perusal of Mayer's works, I was greatly astonished at reading these passages, and my eyes were opened to the evil of writing scientific history in the spirit of a partizan; an evil which cannot fail to recoil upon the writer himself in the loss of confidence it engenders.

Incorrect is no word to apply to these statements. To say they are unlike the reality is to say little. The fact is they are entirely the opposite of the truth; Mayer, throughout his works, exhibits the qualities of the modern physical philosopher, viz., the avoidance of all speculation as to the ultimate nature of things, and the constant appeal to fact, experiment, and exact measurement. He shows the rock of *à priori* speculation on which the ancients split, and more particularly alludes to Aristotle, who, in attempting to explain the lever, took refuge in the assertion that the circle was such a wonderful thing, that it was easy to understand how movement in a circle should produce wonderful effects! But that if instead of wasting his extraordinary talents in meditating on the fixed point and advancing line as he calls the circle, he had simply taken the trouble to ascertain the numerical relation between the length of the lever and the force exerted, he would have laid the foundation of an important department of human knowledge. (p. 240.)

In this way he goes through the chief departments of science, and shows that it was by exact measurement and numbers that all real progress was ever made, and not by hypotheses on the remote causes of the nature of things. And he instances the exceptional perfection which astronomy attained in early ages, as being the result of the observations and measurements of time which are essential to the existence of that science.

It is true that at certain stages of discovery in science we are obliged to fall back on general principles, as Professor Tait in his next paragraph admits, while he says that any hypothesis can only be used as suggestive of further experiments. This is precisely what Mayer does, as has already been expressed by Helmholtz [see note § 13]. The caution of Mayer as a physical philosopher is illustrated by the following remarks, at p. 24:

"If the conversion of heat into work be here laid down, it is meant as the mere statement of a fact, and the transformation itself is by no means explained. A given quantity of ice may be converted into a corresponding quantity of water; the fact stands firm and quite independent of all fruitless questions about the how and why, and of empty speculations on the ultimate cause of the states of aggregation. True science is content with positive knowledge, and willingly leaves to poets and natural philosophers the solution of eternal riddles by help of the imagination."

Indeed, his extreme caution has been blamed, for he hesitates to admit the ordinary physical assumptions which are prevalent as to the nature of matter and force. Tyndall writes—

"On one point Mayer allowed his caution to cripple his philosophy. He evidently feared to have anything to do with *atoms* or their motions, and hence could hardly be said to have realised the complete physical conception of the dynamical theory of heat. Probably, however, many share his caution and adhere to the external facts without seeking to penetrate the molecular actions which underlie them." (*Phil. Mag.*, May, 1863.)

ratio of the effect it produces or the work done; in other words, is transformed into it; then it becomes a most fruitful principle. By means of it Mayer concluded that in the case in point, where work was done by expansion under constant pressure, an equivalent amount of the cause of that work, *i.e.* heat must be consumed; not remaining combined with the expanded gas, to be given out undiminished in the condenser, as Carnot and the rest supposed. This was confirmed by the counter experiment of expansion *in vacuo,* where no work being done no heat is consumed. Here the work was already done when the air was pumped out beforehand, and you no more require the same work to be done twice than you can get the same work twice over out of any power such as waterfall, fuel, or gunpowder. Mayer's conclusions have been confirmed to the very letter by subsequent experiments of others, and the general progress of physical science.

§ 14. In 1844 the now illustrious physicist, Joule, who independently was working in the same direction either simultaneously with, or very shortly after Mayer, repeated the experiments in question, described by himself in the following words: (*Phil. Mag.,* 1857.)

> "It had long been known that air when forcibly compressed evolves heat; and that, on the contrary, where air is dilated heat is absorbed. In order to account for these facts, it was assumed that a given weight of air has a smaller capacity for heat when compressed into a small compass than when occupying a larger space. A few experiments served to show the incorrectness of this hypothesis; thus I found that by forcing 2956 cubic inches of air, at the ordinary atmospheric pressure, into the space of 136½ cubic inches, 13°·63, of heat* per pound of water were produced; whereas by the reverse process of allowing the compressed air to expand from a stop-cock into the atmosphere, only† 4°·09 were absorbed instead of 13°·63, which is the quantity of heat which ought to have been absorbed according to the generally received hypothesis?"‡

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* This amount of heat was gained by a mass of 45 lbs. 3 ozs. of water, 20½ lbs. of brass, and 6 lbs. of iron, composing the apparatus.

† The cold of expansion was produced by the rushing out of 2723 cub. in. of air from the space of 136½ cub. in., which at the same time produced mechanical power enough to raise 3352 lbs. one foot high.

‡ In his original account of this experiment (*Phil. Mag.,* III, vol. xxvi, p.

Again, (Tait, p. 22),

"Joule, repeating in a greatly improved form an old experiment of Gay-Lussac, compressed air to twenty atmospheres or so in a strong vessel, which was afterwards screwed to another previously exhausted. A very perfect stop-cock prevented all passage of air from one to the other until it was desired. The whole was placed in a vessel of water, which was stirred to bring it to a uniform temperature. On opening the stop-cock, the air rushed from the first vessel to the second, so that in a short time the pressure was the same in both. On measuring the temperature of the surrounding water again, *no change was perceptible*, at least after the proper corrections, determined by separate experiments, had been made for the amount of heat produced by the stirring, &c., during the operation."

Thus, these crucial experiments were repeated with the accuracy necessary for exact results; and we perceive there was no loss of heat when no work was done.

As a further test, Joule also observed that when "the second reservoir was full of water, the air in entering was obliged to expel it, and thus perform work, and the temperature sank owing to an absorption of heat." *Ganot*, p. 394.

§ 15. It is desirable now to explain the difficulties which will still be felt by many respecting those phenomena which were formerly attributed to alterations of specific heat. To do so we must anticipate somewhat in detail the theoretical explanation of the nature of heat as a mode of motion, following chiefly Clausius and Tyndall.

According to the dynamic theory of heat, which is now universally adopted, it is supposed to consist in a peculiar oscillatory or vibratory motion of the particles of matter. This motion may be communicated directly from one set of particles to another, or from one body to another, which is called conduction; or it may be transferred by undulations in the æther, or medium, which pervades not only the whole

381) Joule says, "It is quite evident that the reason why the cold in the experiments of table iv [the expansion experiments] was so much inferior in quantity to the heat evolved in those of table i [compression experiments] is that all the force of the air, over and above that employed in lifting the atmosphere, was applied in overcoming the resistance of the stopcock and was then converted back into its equivalent of heat."

interstellar space, but the interstices of ordinary matter; this is called radiation.

§ 16. It was noticed in the experiments of expansion of air *in vacuo* at § 9 and 14 that although on an average no work was done and no heat absorbed, yet in different parts of Faraday's, of Gay Lussac and Joule's cylinders, one portion became chilled, while the other became heated in exactly the same proportion. In this case the particles of air enter the empty cylinder with a certain velocity to generate which a certain quantity of the oscillatory motion, *i. e.* heat, is sacrificed and transformed into the motion of translation, thus causing a certain degree of cooling. But the particles thus rushing forward produce, by their friction on each other, and their impact on the walls of the vessel, an amount of heat exactly equal to the motion of translation which is again annihilated.* Thus we have a certain small amount of work done and heat consumed, which is immediately compensated by the transformation of that work into heat again by friction. This is at once an illustration and explanation of the mode in which heat can be changed into work, and *vice versâ.* It is also plain that the now-doubled bulk of the gas with the same temperature is incompatible with the notion of the increased capacity for heat of rarefied air.

The evolution of heat in the match syringe is also to be referred to the friction of the particles of air upon each other and the sides of the vessel, and being dependent on the force used by the arm of the operator is an instance of the transformation of work into heat. And with an apparatus properly arranged an unlimited quantity of heat may be produced by friction on the same portion of air, which is incompatible with the altered specific heat theory. A difficulty has been raised by the appearance of the cloud in the air-pump at the first few strokes owing to the chilling of the air, as here, apparently, cold is produced while no work is done. But the whole of the work is not done by the arm of the operator, for the gas itself has to do some in raising the valve and pressing out the particles which escape, and this is uncompensated by subsequent impact or friction. Again, the progressive diminution of heat experienced as we ascend from the surface of the earth has been attributed to the mere rarefaction of the air. But this is not the case; for as far as expansion goes, it is performed under the condition of doing the work of lifting the whole superincumbent column of air. There are other causes, likewise, at work, such as the heat communicated from the earth to the lower strata of the atmosphere, and the greater resistance opposed by these strata to radiation from the earth, while the upper strata are much more diathermanous, and allow of a more rapid cooling of the surface.

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* This production of heat by the rushing of air into a vacuum has been incidentally proved and measured by Tyndall in his experiments on what he calls "the dynamic radiation of gases" at p. 343.

§ 17. The expansion of liquefied *Carbonic acid* was noticed at § 8 as a cause of the production of cold, and to a certain extent placed in the same category with the foregoing. But here a different class of phenomena comes under consideration, viz. those referring to the "latent heat" of fluidity and vaporization of solids and liquids, and to their specific heat as contrasted with that of the permanent gases. In the case of solids and liquids a very large quantity of the heat added is neither sensible to the thermometer, nor does it perform any apparent work. In the liquefaction of ice, for example, as much heat is imparted as would raise 79·4 times that weight of water one degree; while to convert a pound of water at 100° C. into a pound of steam, at the same temperature, 537·2 times as much heat is required as would raise a pound of water one degree in temperature. Likewise, when in its liquid state, water requires an immense amount of heat relatively to other bodies in order to raise its temperature, whilst it manifests very little physical change.

By the dynamic theory of heat it is held that in these instances the heat is consumed or put out of existence in doing what is called "interior work" (Clausius, i, p. 23), which consists partly in overcoming the presumed attraction of cohesion, and partly in altering the state of aggregation of the particles, and forcing them into new positions. The nature of these changes and the forces engaged in what is called "interior" work are as yet unknown. And though in many cases an external resistance to these changes is at the same time encountered, as, for instance, to expansion, thus accounting for a certain portion of heat; in others no change whatever is apparent. For example, in the "heating of water from 3½° to 4½° C.—that is, one degree—its volume at both temperatures is the same; there has been no forcing asunder whatever of the atomic centres, and still, though the volume is unchanged, an amount of heat has been imparted to the water sufficient if mechanically applied to raise a weight of 1390 lbs. a foot high."

In all these instances the equivalence of heat to work obtains, and when the temperature sinks, the forces, whose counteraction constituted interior work, again come into play, and their potential energy is reconverted into heat. As expressed by Tyndall, "as regards steam, the heat is consumed in pulling the liquid molecules asunder, conferring upon them a still greater amount of potential energy. When the heat is withdrawn, the vapour condenses, the molecules again clash with a dynamic energy equal to that which was employed to separate them, and the precise quantity of heat then consumed reappears" (p. 144).

Therefore, as a rule, all the heat, communicated to solid and liquid bodies, which is consumed in interior work, is given up again on cooling by a spontaneous and self-acting process. In solid and liquid bodies there are thus three things that heat may effect, 1st, those changes in the aggregation and position of the molecules which is called interior work; 2nd, the overcoming of external resistances to those molecular changes, viz. exterior work; 3rd, the increase of those vibratory motions of the molecules which constitute thermometric or sensible heat.

From the foregoing it appears that in the same category may be arranged the so-called "latent heat" of liquefaction and vaporisation, and also a great part of what is usually called "specific heat," and that, in fact, all the real heat a body contains is the thermometric. Therefore we may consider the *true* specific heat, or capacity for heat, of a body to be that quantity which is required to raise its temperature, independent of what is consumed for interior or exterior work. Accordingly it is maintained by Clausius (p. 267) that although, for example, the specific heat of water is double that of ice, and more than double that of steam, nevertheless, the true capacity for heat of hydric oxide in its three states of ice, water, and steam is the same, and that this rule holds good with all other substances. Therefore, to determine the true capacity for heat of any substance it must be tried in the form of super-heated vapour, *i. e.*, in the state of a perfect gas, in order to avoid the complication of interior work, and at constant volume to avoid that of exterior work. In confirmation of this position I may adduce the observations of Tyndall, who says, " Liquid water and the solid ice are pervious and impervious to the same rays of heat, a result which indicates that the quality of the absorption is not influenced, in this case, by the difference of aggregation" (p. 290).

He extends the same statement to aqueous vapour (p. 405), and after his splendid series of experiments on the absorption of heat by various liquids and vapours, he establishes the law that the absorption and radiation of heat is *molecular* for each substance, and is the same for that substance in the three states of aggregation, viz. solid, liquid, or gaseous (p. 390).

§ 18. It is now abundantly obvious that in the case of solids and liquids, the amount of work done by the heat communicated could not be gauged with sufficient uniformity by the exterior work to render them fit agents for discovery of the mechanical equivalent of heat. The same applies to vapours near their point of condensation, for they have then a certain amount of cohesive attraction, the overcoming of which constitutes interior work. There remain now the non-condensible, or as they are also called, permanent or perfect gases. It is upon the proposition that in these no interior work takes place, that Mayer's determination of the mechanical equivalent of heat rests, for in that case the whole difference between the specific heat of gases at constant volume and at constant pressure, corresponds to exterior work done; and granting that these amounts are accurately ascertained by experiment, his mode of calculating the equivalent of heat is a perfect one.

§ 19. In his first essay of February, 1850, Clausius, after an analysis of the subject, according to all the then known data, concludes that "a permanent gas absorbs, when it expands under constant pressure, only so much heat as is used in the doing of the exterior work thereby performed" (p. 47). This is because any interior work required for the expansion of a gas is inappreciably small (verschwindend klein). Also he concluded that the quantity of heat contained in a gas was independent of its density. These involve a confirmation of Mayer's proposition, but it does not rest on reasoning and calculation alone, for experiment has finally established the truth of it. Regnault, after a lengthened investigation, found "that for pressures ranging between one and twelve atmospheres the specific heat of *equal weights* of a non-condensible gas, such as air or hydrogen, is uniformly the same, and is independent of the density." (Miller, i, p. 309.)

As regards temperature, we know that in all solid and liquid bodies the specific heat increases with the increment of temperature, and Delaroche and Berard (whose authority prevailed when Mayer wrote) taught that the same was the case with respect to the gases; but again Regnault has determined by experiment that for all temperatures between — 30° and + 225° Cent. the specific heat is constant. (*Phil. Mag.* IV, 5, p. 481, and Clausius, i, p. 292.) For example, the same quantity of heat is required to raise the temperature of any gas one degree, whether from 0° to 1° or 90° to 91° or 150° to 151° or any other temperature. The exact determination of the specific heat of gases has been a difficult problem in experimental physics. Again, when Mayer wrote, the estimate of Delaroche and Berard for air, viz. ·267, was held to be more accurate than the previous ones of Crawford, Lavoisier, and Laplace; but great doubts of its accuracy prevailed. Rankine, in 1850, calculated it from Joule's equivalent of heat, at ·24, and in 1852, this was verified by experiment by

Joule. In 1853 Regnault fixed the specific heat of air at constant pressure at ·2375, which is now generally adopted. The determination of that at constant volume is a still more difficult and complicated problem, and has generally been arrived at indirectly. The ratio between the two which is now generally accepted is 1·41 to 1. Finally, it may be stated that Mayer's proposition is admitted by Rankine, Joule, and all the leading physicists of the day.*

§ 20. From the above it may be stated that a gas heated at constant volume exhibits one of the few examples where the true specific heat corresponds to the total heat communicated, and the latter is all employed in raising the temperature, while none is consumed in interior work. On the other hand, when the gas is allowed to expand at constant pressure, the additional heat required is all consumed in exterior work.

This relation of the two specific heats of gases is, therefore, one of the most perfect means of demonstrating and measuring the conversion of heat into work. Hence, we are constrained to admire the marvellous sagacity of Mayer which prompted him to choose this method in the face of all the difficulties then surrounding the subject. We are, therefore, the more astonished that these difficulties have been made the ground of denying to Mayer the merit of the discovery at all. It is said the actual number obtained by Mayer was incorrect;† and, besides, that it was a mere

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* Also the following are among the experimental conclusions of Regnault: "The motive power produced by the expansion of any elastic fluid is always in proportion to the loss of heat undergone by this fluid in the part of the machine where the power is produced." "When elastic fluids of different natures produce an equal amount of power, they must lose equal quantities of heat." (Regnault, *Phil. Mag.*, IV, vol. xvi, p. 269.)

† "Even if he had in 1842 possessed accurate data, and had, therefore, been fortunate enough to obtain an approximate result instead of a very inexact one, his determination could never have been called more than a happy guess founded on a total neglect of correct reasoning." (Tait, p. 18.)

This is another specimen of the injustice of Professor Tait towards Mayer, of whose reasoning the reader can judge for himself, and whose deductions have been confirmed by experiment to the very letter.

We observe throughout his work that Professor Tait continually couples the names of Séguin and Mayer together as if their claims were equal, evidently assumption that the whole work of compressing a gas was converted into heat, as it might (Tait, p. 34) have been employed in overcoming interior forces, and thus stored up as mechanical power. This last has already been answered

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for the express purpose of disparaging Mayer; a proceeding surely unworthy of a successor of Leslie in the chair of natural philosophy. A few words on this subject may prove not unwelcome to the reader. In 1839 Séguin, nephew of the celebrated Montgolfier, from whom he is said to have got his ideas about heat, published a book upon the political economy of railways, which contained some remarks on the theory of the steam engine. Among these are the following:—"*I believe I have remarked* that there exists *a kind of relation* between the heat and force produced;" and again, having remarked on the difficulty of the ordinary material theory of heat to account for the work while the quantity of heat is undiminished, he says, "It appeared *to me more natural to suppose* that a certain quantity of heat disappeared in the production of the power, and reciprocally; and that the two phenomena are bound together by conditions which assign to them invariable relations." Then he goes on to say that the fact of the cooling by expansion of a gas, and the heating by compression, tend to confirm that opinion, and continues, "Je vais donc raisonner dans l'hypothése que l'abaissement de temperature de la vapeur lorsquelle se dilate, represente exactement la quantité de puissance qui apparait alors." (J. Bertrand, p. 672.) In another place, according to Tait, he says that, when steam has done work, it should not heat the condenser so much as when none is performed. From these it appears that Séguin is entitled to great credit, as he was probably the first who apprehended the idea theoretically. But he did not discover the Mech. Eq. of Heat, nor push the matter to a practical conclusion; and as his book was not on physical science his speculations remained barren of result, and would have lain unknown but for the discovery of Mayer and Joule. In the *Phil. Mag.*, IV, vol. xxiv, Mr. Joule makes the following remarks upon Séguin:—"He (Séguin) gives a table of the quantity of mechanical effect produced, corresponding to the loss of temperature of steam on expanding. From this it appears that 1° Cent. corresponds with 363 kilogrammétres." At the time of the Mayer controversy in 1863-4 (§ 38, note), this statement attracted the attention of Tyndall, who referred to the original work of Séguin, and after quoting the above table, showed that it merely exhibits the expansive power of steam corresponding to a given temperature. Thus in one column of the table headed, "Effet produit en kilogrammes élevés à 1 métre,' the first figure here is 7270 kilogrammes, and under "temperature correspondante" stand 20°. Now, by dividing 7270 by 20, we certainly get 363, and so on the rest. Thus we would apparently get the mechanical effect due to a single degree. "All this," says Tyndall, "seems very plain; and did no text accompany the table, and had not M. Séguin in that text explicitly defined his own terms, we might be justified in assuming that he meant the number 363 to stand for the M. E. H. in the same sense as Mayer did. It is only necessary, however, to read the foregoing pages to see that Mayer and by Mayer's reasoning from the expansion *in vacuo*, which is so convincing that it amounts to simple common sense; and lest any ambiguity should remain from some exceptional power of air storing up potential energy in interior work, as was the case with the freshly-heated iron nail that misled Black (§ 8), that was met by adducing the law of Dulong at § 12. And in respect to the erroneous figures,*

Séguin are speaking of two totally different things; that the degrees of the one are not the degrees of the other; that the "temperatures correspondantes" of the latter, which refer to his compressed steam, are not thermal units at all; and that there is no determination whatever of the M. E. H. in the above table. The number 363 has been found *for* Séguin, not *by* him; he never made the division which results in this quotient." In consequence of this explanation Joule, in August, 1864, frankly admits his mistake in the following words:—"After the perusal of this article I freely admit that I erroneously took the degrees in the fifth column of Séguin's table for thermal units in kilogrammes. I regret this oversight, the more particularly as it seems to have misled others who have since written on the subject." Tait, however, returns to the charge, and finds some other data in Séguin from which he (Tait, not Séguin) calculates the M. E. H. and makes it 650. Tyndall, however, states that afterwards, viz. in 1847, Séguin gives a calculation of his own, without explaining how he did it, and gets as the M. E. H. a number from 395 to 529. His data were afterwards shown to be erroneous, and Regnault's subsequent experiments on the specific heat of steam defeated his calculations. Then his friend, Mr. Grove, repeated them with Regnault's data, and got as the M. E. H. 1666. It is, therefore, abundantly obvious that Séguin did not discover the M. E. H., and according to Verdet no such claim is put forward in his behalf by his countrymen, who are not generally thought to be backward in claiming at least their own share of the glory of discoveries.

* Tait implies that Mayer's results are beyond the limits allowable for experimental error. This opinion does not appear to be shared by other physicists. At any rate that prince of experimenters, Joule himself, does not agree with it, in respect to his own results. For at p. 41, *Phil. Mag.*, 1843, he says of his own original first thirteen experiments, "I admit there is a considerable difference between some of the results, but not I think greater than may be referred with propriety to mere errors of experiment." These results varied from 587 to 1040 ft.-lbs., and the average was 838. This constitutes an error of 8½ per cent. too high, which Tait, forgetting his rôle of counsel against Mayer, pronounces to be wonderfully accurate for a first series.

Again, Joule, in *Phil. Mag.*, 1842, Febuary, in a paper on the relative heat of combustion of different elementary bodies, found the experimental result 25 per cent. below the calculated amount. After enumerating several practical difficulties, he concludes, "It will, I think, be admitted that experiment agrees with the theory as well as could have been expected."

that cannot be held to vitiate his claim as a discoverer, for he announced the convertibility of heat into work at a constant numerical ratio, the determination of which was liable to the usual chances of experimental error. A similar style of reasoning would invalidate the doctrine of the indestructibility of matter, for no chemical analysis has ever yet given 100 per cent.

§ 21. We now pass to the second mode of proof alluded to at § 4, viz. the more direct mode, by measuring the quantity of heat produced by the expenditure of a known quantity of work in friction. This method required the use of complex and delicate machinery, as well as the uninterrupted attention and the technical skill of the professed physicist, all of which were beyond the means and opportunities of a young surgeon in the first year of his practice. But if he had neither the means nor the apparatus, he had, like the celebrated painter, brains in full measure. So he cast about him for a suitable instance of the incidental production of heat in the machinery subservient to the ordinary wants of life. Had he not the heating of the millstones, and the flour in flour-mills, and of the rollers and seeds in oil-mills, and the perpetual evolution of heat in the axles of all moving wheels, the heating of cannon while boring? &c. But none of these seemed so well

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Now, Mayer's first calculation was 14 per cent. too low, owing to the experimental error, not of his own, but of Delaroche and Berard.

Although Mayer's claims do not rest upon experiments performed by himself, yet it is not quite correct to say he made none, for he alludes to several, one for instance respecting the development of heat by the union of *Chlorine* with *Ammonia*, p. 47. And at page 284 he mentions his invention of a dynamometer which he hopes may compete with that of Prony.

The following expressions of Mayer himself are apposite, if restricted to his experiments alone, although with the modesty often united to true genius he extends them to the whole of his labours.

"The proof of this conclusion by the method of experiment, the establishment of it in all its details, the tracing of a complete harmony subsisting between the laws of thought and the objective world, is the most interesting but also the most comprehensive problem that it is possible to find. What I have accomplished in this direction with feeble means, and without the slightest encouragement or support from without, is certainly not great, but *ultra posse nemo obligatus.*" (p. 255.)

adapted for the purpose as a machine which involved fluid friction, in spite of the dicta of the text-books of chemistry which denied the production of heat to the friction of fluids. Accordingly, he turned his attention to a neighbouring paper mill. At this very time, though unknown to each other, Joule was commencing his now celebrated experiments with fluid friction, though under very different circumstances; for he was trained in the school of Dalton, and had sufficient means and appliances at command. The following is Mayer's observation given in his own words:

> § 22. "I made observations on four pulping cylinders in a paper mill. In each cylinder was contained about 80 lbs. of paper pulp, and about 1200 lbs. of water. The temperature of the pulp rose steadily from the time of setting in. The surrounding temperature showed 15° Cent.; in thirty-two to forty minutes the heat of the pulp rose from 14° to 16°. The highest temperature observed, which was uniformly maintained for several hours before drawing off, amounted in one cylinder to 30°. If we now reckon that by one horse-power, about 27,000 lbs. are raised one foot high in one minute, then the heating of 1280 lbs. of water (not counting that of the apparatus), 1° in sixteen minutes must be the equivalent of 3·16 horse-power, which agrees with sufficient accuracy with the computation of the manufacturer, viz. that a total of about five horse-power is required for driving one cylinder. Now, does the mechanical effect yielded by the five horses vanish into nothing in the machine? The fact speaks—*it becomes heat.*" (page 37.)

This observation is not put forward by Mayer with any pretensions to the rigid accuracy required for determining the mechanical equivalent of heat, but is merely adduced as a counter-proof of the foregoing method by gaseous expansion.*

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* This is exactly parallel to Rumford's celebrated experiment on the boring of cannon, in respect to which Joule has the following remarks in the paper noticed at § 34. "Referring to his third experiment, he remarks that the 'total quantity of ice-cold water, which with the heat actually generated by friction, and accumulated in two hours thirty minutes, might have been heated 180°, or made to boil = 26·58 lbs.' In the next page he states that 'the machinery used in the experiment could easily be carried round by the force of one horse (though to render the work lighter, two horses were actually employed in doing it.') Now, the power of a horse is estimated by Watt at 33,000 foot-pounds per minute, and therefore if continued for two hours and a half will amount to 4,950,000, which, according to Count Rumford's experiment,

§ 23. Such is an outline of the development of Mayer's discovery as a contribution to physics. He next proceeded to its application in the more congenial field of physiology, where, it soon appeared, it was destined to work as great a revolution as in physics. This was done in the work called *Die Organische Bewegung*, published in 1845, which in reality should be considered the first work in which he had the opportunity of properly explaining his views; for "the forces of inorganic nature," analysed at § 2, may be regarded as a mere preliminary note, hurried forward to secure priority, and cramped for want of space. He now fully develops the doctrine of the conservation and equivalence of force in the widest sense in which it is at present understood, and among numerous calculations and illustrations, some of which have been given above, he selects twenty-five experimental examples of the transformation of the different physical forces into each other. In the latter part he uses the mechanical equivalent of heat to measure the forces engaged in vital processes, and lays the foundation of the doctrine of the balance of forces in the living economy which characterises the most modern school of physiology, represented in this country by S. Haughton, E. Smith, Frankland, Parkes, and others.

To give all that is interesting in this part would simply be to translate the whole essay; but as a specimen, I may note the application of the principle to the antagonistic or complementary position of heat and work in the living body. In the inorganic part he gives that as respects steam-engines, gunpowder, &c., and states that in the former, probably, scarcely more than 5 per cent. of the total heat is used as work; while in the latter about 9 per cent., and makes here the acute remark that a gun fired with ball cartridge *heats* less

will be equivalent to 26·58 lbs. of water raised 180°. Hence the heat required to raise a lb. of water 1° will be equivalent to the force represented by 1034 foot-pounds. This result is not very widely different from that which I have deduced from my own experiments related in this paper, viz., 772 foot-pounds; and it must be observed that the excess of Count Rumford's equivalent is just such as might have been anticipated from the circumstance, which he himself mentions, that "no estimate was made of the heat accumulated in the wooden box, nor of that dispersed during the experiment."

In reference to this, Tait says the horse-power is now taken at 30,000 ft.-lbs., which would bring the M. E. H. to 940 ft.-lbs.

than with blank cartridge—a remark we shall see paralleled in physiological processes. On entering the subject the genius of the true philosopher shines out conspicuously in the following words:—'As to the *how* a muscle effects the metamorphosis of chemical difference into mechanical action we cannot say. In numberless instances the transformations of matter and force in the inorganic and organic worlds take place before our eyes, and yet each of these processes contains a mystery impenetrable to human understanding. The sharp definition of the limits of human investigation is a problem of practical value for science, while the efforts to penetrate by hypotheses into the depths of the order of the universe form a foil to the striving of the adept.' (p. 99.)

By keeping in mind these general principles he, in 1845, laid the foundation on which has been built the distinctive part of modern physiology in such a way that, though much has been added, little has been changed. Though he recognises the great complexity of the chemical processes in the living organism, yet, as most of them are too little known for calculation, he has to fall back on the oxidation of the carbon as the source for quantitative determination on which his calculations rest, observing that in future they can be altered by simple reduction when the necessary data are discovered. This, and the M. E. H., being 14 per cent. too low, give a certain error in amount to all his calculations, but the principle remains the same. Now, in the development of the subject which has since been made, the moving force of all vital actions is expressed as not consisting in oxidation alone, but in the disengagement of active force by all chemical processes in which stronger affinities than before are satisfied. Thus, force becomes free in the form of heat when sugar falls down from the complicated composition $C_6H_{12}O_6$ to the simpler compounds 2 of *Alcohol* and 2 of *Carbonic acid*, $2C_2H_6O$ and $2CO_2$. The number and variety of such transformations is great, and we can imagine a considerable amount of force extricated in vital processes without the aid of extraneous oxygen at all. But we can easily see that such would be a most wasteful life, as nothing but a full supply of oxygen can extract the whole potential energy by bringing the organic matter down to the simplest compounds possible, viz. water, *Carb. acid*, and urea. Likewise these transformations are not all downwards, but some are synthetic, *i.e.* built up from simpler to more complex compounds, such as protagon and hæmoglobin, which thus require the conversion of free into potential energy. Therefore, though some vital processes are independent of oxidation, yet it is essential to maintain the average amount of force, and as no kind of transformation can possibly equal the disengagement of force due to oxidation down to binary compounds, it becomes the absolute measure of the disengagement of force in the living body, and the law is thus expressed: viz. the quantity of *vis viva* developed (permanently) by the living body must be equal to the difference between the heat of combustion of the food and the heat of combustion of the excretions. Again, the vital processes resulting in the excretion of *Carbonic acid* and those in absorption of oxygen, though they exactly balance, are parallel and independent, not the same act, nor even necessarily simultaneous; *e.g.* in muscular exertion the surplus amount of exhaled *Carbonic acid* over that in repose is much greater than the absorption of oxygen in exertion exceeds that during repose (Pettenkofer and Voit). Likewise there is no proof that the combination with oxygen, nor any single metamorphosis in the *living matter*, is of a kind known as chemical; but the final result as to extrication of force must be exactly the same; for a given quantity of matter of a certain complex composition passing down to known simpler binary or ternary compounds must give up the same force by whatever road it travels. Now, to return to Mayer. In mammals, at least, the whole free force extricated comes out in the form of heat or mechanical work; so he proceeds to apply his measure of value to certain vital actions with remarkable and interesting results. He it was who first calculated the share of work and heat which go to the chemical changes involved in the daily and hourly work of a horse, and explained the difference of quantity of food required for the animal when at rest and at work. In like manner he shows that for a man there is consumed in work 0·19 lbs. of carbon a day, or 3·2 grains a minute. A bowler throwing an 8-lb. bowl with a velocity of 30 feet per second expends the force of $\frac{1}{10}$ grain of carbon oxidized. In climbing a mountain 10,000 feet high, a man consumes about $2\frac{1}{2}$ oz. of carbon, and so on.

But work is not the only force-product of oxidation; there is also heat. And it is plain that as the total amount of force producible by the chemical change is fixed, what is used for work must be taken from heat, and *vice versâ.* This is in accordance with experiment, and calculation, and common observation. Muscles at rest are, in respect to heat, like other parts, but at each contraction heat in the nascent state becomes latent, *i. e.* converted into motion; and if now the arterial circulation and the chemical processes are not increased in equal proportion, then the disengagement of heat must be *less* than at rest. At p. 107 he calculates that the loss of heat in the mass of the voluntary muscles must amount to $\frac{1}{100}$th of a degree C., and therefore, after working, a perceptible decrease of heat must take place. Accordingly he finds reported by Douville that the heat of a negro working in the sun stood at 39·75, while at rest it amounted to 40·2.

But in almost all cases during exertion there is increase of general circulation and change of matter; so the total heat is increased. The active part becomes turgescent, and a vein opened there would pour out more blood. The respiration and the heart's action are increased, owing to the increased demand for oxygen. Hence, in estimating the increased amount of pabulum consumed in forced labour, we must take into consideration the increase of heat necessarily produced. After an elaborate calculation of the ratio between food, work, and heat, in horses, prisoners, and soldiers in the different conditions of labour and rest, he arrives at the following conclusions:—"1st. The greater consumption in the working organism of combustible food suffices, even keeping in view the *plus* of heat generated, to account for the production of mechanical effect in the natural way. 2nd. The carbon expended by mammals at forced labour in the production of mechanical work amounts at the maximum to only one fifth of the total expenditure; the remaining four fifths are used for heat production" (p. 71). These calculations have since been confirmed. Edward Smith measured accurately the carbonic acid exhaled in motion and rest, and found it five times greater during forced labour than in repose; and, according to Hirn, during forced labour the hourly absorption of oxygen is increased five-fold. Upon this Helmholtz makes the following calculation:—The heat produced by chemical processes in the human body during one hour is about 1°·2 Cent. This would raise its weight to a height of 1760 feet. We could by severe walking do this in an hour; therefore the work done in forced exertion is equal to the ordinary heat of repose. But the heat-production in exertion is five times that of repose; therefore one fifth goes to work, and the other four fifths cause increased heat.

In spite of these obstacles, Mayer brings some examples where the antagonism of heat to work has become matter of common observation. "Daily experience teaches this. It is a well-known saying that to get far you must begin with slow steps. The proverb says, *festina lente.* The workman seeks to avoid perspiration to save his strength, and the driver does not like to see his horses *heat.* In common language it is said, Sweat wastes strength." (p. 68.)

And, again, while noticing the general increase of temperature from exertion, and consequent sweating, he says, "*But it is remarkable that the parts in the greatest state of activity are wont to sweat the least.* Robust peasant girls, whose hands get into profuse perspiration by knitting and the like, can do heavy field-work without the skin of their hands and arms becoming even moist. The sweating of parts like the head, which do no mechanical work during bodily labour, is alluded to by Moses when God speaks to Adam—'In the sweat of thy face shalt thou eat bread.'" (p. 110.)

Mayer's observations have been confirmed by actual experiment, for Becquerel, Breschet, and Helmholtz have shown that the changes of matter in muscles can produce heat, and Haidenhain has demonstrated that the amount of heat stands in relation to the mechanical work done by the muscle, so that when it contracts it extricates less heat the heavier the weight that is raised by its contractions. (Kühne, *Physiol. Chemie*, p. 313.)

The following experiments are also given by Professor

Fick:—"I caused a frog's muscle to perform a determined number of contractions twice in succession under exactly the same conditions, except with the difference that in the first series there was a weight actually raised by each contraction, whereas in the second no mechanical work was done. By a delicate thermo-electric apparatus the heat evolved in the contractions was determined, and it was shown, in fact, that *more* heat was evolved in the second than in the first series. (p. 58.) Billroth and Fick found that the high heat of tetanus was produced in the muscles, and in cases of tetanus the muscles were hotter than the rest of the body, the heat in fatal cases reaching to 111° Fah. This confirms also another remark of Mayer's, that in delirium, epileptic convulsions, and in the tricks of jugglers in which muscular action takes place without doing real work, little expenditure of force takes place, and in trismus almost none. So the effect comes out as heat (p. 123). We know also that in the state of rest muscles develope a current of electricty which is immediately brought into equilibrium, and most likely becomes heat; but as the muscle becomes active the electricity diminishes, and on full action ceases altogether. We thus again see the reciprocity of work and other forms of *vis viva.* Here, as elsewhere, when phenomena are found difficult of explanation, electricity has been conjectured to be the intermedium of the change of chemically evolved force into work, but no proof has as yet been offered. We sometimes also hear it loosely stated that the animal heat is converted into the mechanical work in the muscles. Mayer gives no countenance to such a mode of expression; and he merely speaks of heat in the *status nascens* being so used, implying the force or energy which might or would become heat if not otherwise used. It happens, as Fick remarks, that we can positively prove that the muscles are incapable of producing mechanical work from heat in the manner of a thermo-dynamic engine. For we know that of the total heat producible by the animal body one fifth can be converted into work; but Clausius has shown that in a thermodynamic engine it is not possible to get one fifth of the disposable heat, converted into work, except when the remaining four fifths pass down from a very high temperature to a lower one. No such temperatures are ever approached in animal bodies, and therefore muscular work cannot be done by heat. (p. 59.)

Mayer next, with extreme ability, deduces three important propositions by means of the quantitative relations of the chemical changes and evolved *vis viva.* In these, although he does not anticipate all the discoveries in matters of detail which have been since made, yet he maps out the boundaries within which the truth must lie.

1st. "The muscle is only the instrument through which the transformation of force is brought about, but it is not the matter by the metamorphosis of which the effect is produced." This he proves by calculating the quantity of carbon required to be burned to obtain the equivalent of the known work of the muscles, and shows that if it was derived from the muscles, their whole mass in a labouring man would be consumed and require renewal in eighty days. But the case is much stronger with the heart, and according to the data of Valentin and Poiseuille, the ventricles would be consumed in three and a half days. This is totally opposed to the known pathological and microscopic facts respecting the renewal of tissue.*

2nd. Although the amount of work of a single contraction is, in proportion to the mass of a muscle, owing to the chemical changes of the matter contained in it, yet "the enduring power of doing work is proportional, not to the mass of the muscle, but to the mass of the blood circulating through it" (p. 105). This he shows by calculation and examples such as this:—If we stand on one foot, by the muscles of the calf we can easily raise the body one and a half inch. This about equals the work of one systole of the ventricles of the heart, but if you try to do this isochronously with the pulse, you will very soon have to give up the competition. The reason is the enormously greater supply of arterial blood to the heart, which gives the needful store for renewal of the change of matter furnishing the force.

3rd. "Not the hundredth part of the oxidative process is performed outside the walls of the blood-vessels" (p. 89). This he shows thus:—Nutrition is carried on by imbibition of the interstitial exudation from the blood-vessels, but can the quantity of oxygen carried by this exuded fluid suffice for the chemical changes equivalent to the work done by the muscles? On measuring it by Majendie's calculation of the quantity of the lymph, it is wholly inadequate. As to how force set free in the capillaries can be transformed into work in the muscles, he makes no attempt to explain, but leaves it as a problem as yet inscrutable.

Since then (1845) much has been done in this field, and nearly all is in harmony with the above. The 2nd is amply confirmed in every theory now obtaining. The 1st completely demolished the theory of Liebig then in vogue and which has been disproved in many different ways since; such as the demonstration by Voit, Fick, and Wislicenus that the urea is not increased by muscular exertion. The 3rd, however, is a more difficult question, and one that has scarcely yet received full attention. Mr. Heaton has written upon the subject (*Phil.*

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* Beale thinks it "not improbable that in the higher vertebrata the very same elementary fibres continue in action for years," and adds "the idea that the contractile material could be removed or replaced in a few days or weeks could only be supposed by those who were ignorant of or determined to ignore the results of anatomical investigation altogether." (*Med. Times*, Feb., 1869.)

Mag., May, 1867), and after repeating Mayer's calculation, with the more correct data of Bidder and Schmidt, in respect to the lymph, comes to the same conclusion that the exuded fluid cannot possibly furnish nearly the quantity of oxygen required. Now, besides being incompatible with the theory of Liebig, Playfair, and Ranke, this also goes against that of Traube, Fick, and Wislicenus, who derive muscular power from the oxidation of the fats and carbo-hydrates also outside the vessels; and of Donders, who attributes it to both classes of compounds. But Dr. Broadbent, in July, 1869, answers that it is an assumption that the oxygen can only leave the capillaries in solution in the exudate. On the contrary, by the laws of diffusion an interchange of oxygen and carbonic acid can take place through the capillary walls without any current of fluid, just as happens in the lungs, and such being the case, the arguments for extravascular oxidation predominate. This may be, but still it does not settle the question, and the nature of the systemic blood change (or internal respiration) is still involved in difficulty, and it is most likely that, as above said, direct oxidation does not take place at all. This was not apparently distinctly apprehended by Mayer; but if we admit that, after all his principles may be found in harmony with the most recent theory of muscular action put forward chiefly by Ludimar Hermann.* The following is his view of the chemical changes involved in the extrication of force in muscular movement:—"The muscle contains at each instant a store of a complicated nitrogenous substance, 'inogen,' dissolved in the muscle-contents, which is capable of a splitting† up with evolution of force; the products of this splitting up are, among others, carbonic acid, lactic acid, and a contracting albuminous body, myosin. The splitting up takes place slowly during rest, spontaneously; suddenly at the temperature which produces heat-tetanus; it is also suddenly augmented by 'stimuli;' this sudden augmentation is the essential point in the state of activity. When this substance is exhausted there is no more muscular action possible" (p. 226). This substance, "inogen," has not been isolated, because it splits up as above by every chemical treatment as yet tried. . A continual restitution of this substance must be taking place, as well as the removal of the products, and it is found that *Carbonic* and *Lactic acids* speedily impair and stop muscular action, though urea does not. Hence the necessity of rapid circulation. The restoration takes place by the synthesis of the inogen from oxygen and another not isolated or named non-nitrogenous substance, and the myosin. The latter, therefore, remains, and thus undergoes a kind of chemical circulation. This theory would account for the chemical change sufficient for the required *vis viva*, also for the occurrence of contraction for a short time without oxygen, and even out of the body, while a large supply of arterial blood is essential for its *continuance;* for fatigue from predominance of exhaustion over nutrition, also for the exclusive consumption of non-nitrogenous materials in muscular action, and for the acid reaction of muscles in activity.

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* *Grundriss der Physiologie.*

† Spalten—to split up—is applied to the decomposition of compounds without oxidation.

The main point in any theory is to account for the quantity of *vis viva* evolved even if we cannot show the method of the conversion of it into work which is not done here, and in all theories that still remains hidden. The weak point in this chemical theory is also to explain the action of the stimuli which we cannot understand as producing even the necessary decomposition, far less the oxidative synthesis of the inogen. That can only take place by a vital process, and most likely through that usually ascribed by Beale to the masses of protoplasm of the secreting organs. In the case of muscular motion he considers the act of contraction a function of the non-living part, but he (in the Oxford lectures, 1869) gives no theory of the mode, nor does he account for the quantity of force evolved. But he agrees with Brücke and Kühne's view of the nature of the primitive fibre, viz. that it is not a fibre at all, but a tube of sarcolemma containing a fluid,—the plasma which coagulates after death into myosin and serum,—and the flesh-prisms,*—soft solid bodies maintaining a definite arrangement in an unknown manner (*Physiol. Chemie*, 282). It is the alteration of position of the flesh-prisms which produces normal contraction while the coagulation of the myosin produces *rigor mortis* (Brücke, Kühne). Hermann, on the other hand, holds healthy contraction, tetanus, and *rigor mortis* to be all degrees of the same process, which is a very weak point in his theory.

In fine, as any theory must show sufficient chemical change and consumption of oxygen, and as the transference of abstract "force" from an oxidation performed in the capillaries is not thinkable; if Mayer's theory is to be reconciled with subsequently discovered facts, it seems to involve the assumption that the formation of any such substance as inogen must take place in the capillary walls. We know that during the circulation through the arteries the blood loses some of its oxygen, and, even in the blood out of the body, the white corpuscles continue to undergo vital development with absorption of oxygen for a time. Now the outside of the sarcolemma is studded with masses of protoplasm which, according to Beale, belong to the nerves and capillary arteries, chiefly the latter, which masses were before him erroneously looked on as connective-tissue corpuscles. We can, therefore, easily conceive these to produce the oxidative synthesis of the inogen or any similar substance whose splitting up is to furnish the needful *vis viva* for muscular functions. At the same time, by a perfectly independent process, though simultaneous, the products, viz. carbonic and lactic acids, &c., are absorbed into the capillary veins. We have, thus, an oxidative synthesis within the vessels by a vital process, whereas a purely chemical process, consisting of simultaneous oxidation and splitting up in the non-vital sarcolemma-contents under the influence of stimuli is not conceivable. Against any theory of combustion it must also be kept in mind that the evolution of carbonic acid exceeds the consumption of oxygen in active muscles, the venous blood of which contains 3 per cent. less oxygen and 4·1 per cent. more carbonic acid than that of muscle at rest (Hermann, p. 135).

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* The flesh-prisms (or sarcous elements of Bowman) are found in both kinds of muscular fibre, and it is to their more regular arrangement in the striped muscles that the characteristic appearance of these is owing. They are not found at all in the true protoplasm, *i.e.* the living or germinal matter of Beale, who accordingly ascribes its movements to a totally different cause. The very fluid state of the contents of the sarcolemma was proved to Kühne by the accidental entrance of a parasite into a fibre which he was watching. It moved about with perfect freedom, and even was seen to displace some flesh-prisms even to a right angle, showing they were not fibres extending from end to end, among which it could move like a snake in standing corn. What restores them to their place and keeps them there is not known. (Kühne, p. 281.)

Another interesting point treated of by Mayer is the distinction between fatigue from real work and merely profitless though irksome muscular movements. In the former case there is the expenditure of a real something, viz., in lifting upwards or moving otherwise our own or any other body, we consume a measurable object equal in the former case to $mh.$, *i. e.* the product of the mass into the height, and in the latter $mv.^2$, *i. e.* of the mass into the square of the velocity. This is equivalent to a known quantity of food transformed. So the appetite should correspond to the work done, and not to the mere sense of fatigue from over tension of the nerves. Mayer distinguishes between these and gives the example of a student who may be wearied with standing all day at a desk, but will walk smartly up a hill in the evening for recreation. Also by holding the arm in an irksome position for long, or making purposeless movements wherein the nerves are fatigued, the muscles recover at once with a little rest, and the rest of the body is not affected, whereas in real over fatigue from walking or working, not only the limbs exercised, but the whole body is incapable of further exertion. In my medical practice these observations have been forced on me as matters of fact, and I have been accustomed to impress the necessity of real walking exercise on idle self-indulgent people who complained of want of appetite, while, at the same time, they were "always tired" with their household occupations. I have hitherto been accustomed to tell them that nerve-fatigue was not necessarily exercise, without being able to explain exactly the reason.

I may finish this part of the subject by merely noting that Mayer first pointed out the true relation of light to the function of plants, and quoting Tyndall's dictum on his services to physiology.

"Thus does this remarkable man, at a time when the writings of the most celebrated scientific professors were beset with mysticism as regards the operations of the vital force, pour light upon the darkness and bring the processes of the animal body into harmony with the great law of conservation, which he himself, alone and unaided, had thought out."—(*Phil. Mag.*, p. 41, July, 1864.)

§ 24. In the foregoing works Mayer of necessity makes frequent allusions to the forces governing the motions of the heavenly bodies. He now proceeds to apply to them his new-found measure of force, which like another telescope has opened up for us sources of knowledge, the possibility of which had scarcely even been hoped for. In 1848 he published his *Dynamik des Himmels*,* or *Celestial Dynamics*, a work which, if possible, surpasses his previous ones, and of whose merit no words of mine can give any adequate idea; it must be read and studied in order to be appreciated. But I cannot refrain from a few reflections on his career during these seven eventful years of his life. We see him in 1840, as a ship's surgeon in Batavia, struck with the unusual colour of the venous blood; and from that moment he never loses sight of the fact. He ponders deeply over it, and follows up the train of reflections inspired by it during the tedium of the return voyage. Instead of yielding to the languor induced by the monotony of the passage in the trade winds, when the sails are not perhaps shifted for days, or the sultry calms of the equator, his mind is ever active. He watches the flight of birds, the course of fishes, the direction of currents and winds, and, in short, stores his mind with the facts of physical geography which are, seven years later, to serve him in such good stead in his *Celestial Dynamics*. No doubt during this time the grand conception of the conservation of force was gradually maturing itself in his mind, and immediately on his settlement at Heilbronn he proceeds to submit his hypothesis to the test of extant experiments and ascertained facts to which, most probably, he had previously had no access on board ship.

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* Translated in the *Phil. Mag.*, IV, vol. xxv.

His first paper contains in reality the whole doctrine in a compressed form, and the others may be looked upon as merely developments of it; his whole works, in fact, read like one book, so continuous is the train of reasoning. The solidity and boldness with which he grasped the full doctrine from the very first distinguish him above all others, and form a great contrast to the hesitating and tentative way in which Grove, Carpenter, Faraday, and even Joule himself, first touched upon the subject. With marvellous rapidity he discovers a principle which shall profoundly influence physics; he then applies it to physiology, and begins a new era therein; then carrying it into astronomy, he finally wrings from the heavens what Herschel calls 'the great secret' of the source of the sun's heat and light. When we reflect that hitherto the progress of chemistry has only served to deepen, if possible, still further this apparently unfathomable mystery, we are almost overwhelmed with the greatness of the discovery which has brought it within the range of legitimate scientific speculation.

§ 25. It would be out of place to go into the meteoric theory of the sun's heat in detail in this place.* It may very shortly be stated that he first shows the total impossibility of any chemical means such as combustion either producing or maintaining the sun's heat and light. Next he shows the competency of mechanical causes to do so. Supposing the matter of the sun falling together from an immense distance with the entire pull of gravitation, sufficient heat would be generated to supply the sun at its present rate of emission for 20,000,000 years. So far—viz. as regards the origin of solar heat—all subsequent writers agree with Mayer; and their opinion is summed up by Sir W. Thomson as follows :—"Meteoric action is not only proved to exist as a cause of solar heat, but it is the only one of all conceivable causes which we know to exist from independent evidence" (*Age of the Sun's Heat*). But there is not the same agreement with respect to the sustenance of the sun's heat. Mayer showed that the numerous meteoric bodies continually traversing our system could, by falling into the sun, keep up its heat; and he thought it probable that this was the cause. Mayer included meteoric matter from all parts of the solar system, but thought the chief source to lie in that zone of meteoric matter known as the zodiacal light, and which has been termed by Herschel "an illuminated shower, or, rather, tornado of stones." However, W. Thomson showed that no material quantity of meteoric matter to feed the sun's heat can come from beyond our earth's orbit, because the increased solar attraction would have already produced a perceptible shortening of the year, which has not been confirmed by history. Therefore any such meteoric matter must be already within our orbit, and, he thinks, very near the sun. But Leverrier has further found that from the slight disturbance of the planet Mercury, any such matter must be comparatively small in quantity, and insufficient to keep up the heat. The same is confirmed by the undisturbed passage of such attenuated bodies as comets very near the sun.

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* Those who have not access to the original will find an account of Mayer's Meteoric Theory of the Sun's Heat in Tyndall's book on *Heat*, pp. 477—483. An abstract is also given in *Knight's British Almanack* some five or six years ago. Two most interesting papers on the same subject are given by Sir W. Thomson, one in the *Phil. Mag.*, IV, vol. viii, and the other in *Macmillan's Mag.* for March, 1862, entitled "Age of the Sun's Heat." In the first of these Mayer's name is not mentioned, and in the second the only allusion to him is that the meteoric theory "appears to have been first proposed in a definite form by Mayer." Of course, after the discovery of the mechanical equivalent of heat it was open to any one to make these calculations for himself; but one would have thought that the first complete essay on the subject, such as Mayer's, which had been published fourteen years before, merited something more than that scanty acknowledgment. In 1853 Mr. Waterston, apparently independently, sketched out a similar theory, and of this the above paper of Sir W. Thomson is said to be merely an expansion.

Other speculations more or less plausible are given by W. Thomson, Helmholtz, and others, but they all rest on the production of heat by mechanical causes. However this question may be finally settled, we perceive that Mayer has gathered the first fruits of the application of the mechanical equivalent of heat to astronomy.

I may not inappropriately conclude this sketch of Mayer by saying that the *Celestial Dynamics* has received its meed of general admiration. J. Bertrand, after giving an analysis of it, speaks of it in high terms of eulogy; and Tyndall terminates his notice of it in these words :—

"But the sketch (of the meteoric theory, &c.) conveys no adequate idea of the firmness and consistency with which he has applied his principles. He deals with true causes; and the only question that can affect his theory refers to the quantity of action ascribed by him to these causes. . . . It is a noble speculation; and depend upon it, the true theory, if this or some form of it be not the true one, will not appear less wild or astonishing." (p. 483.)

§ 26. Let us now turn to Joule's share in this great work. He, like Mayer, went through a preliminary stage, and the experimental investigations on which he was engaged, dating from 1840, had all a bearing on the discovery which he made in 1843. After a series of researches undertaken in the former year, tending to show the existence of a ratio between the heating, chemical, and mechanical powers of the Voltaic apparatus, he published in 1841 a paper entitled "On the Heat evolved by Metallic Conductors of Electricity, and in the Cells of a Battery during Electrolysis." (*Phil. Mag.*, III, vol. xix, p. 260.) Now, like Mayer, true to the principles of modern physical philosophy, instead of referring that to the properties of any hypothetical essence common to them, he seeks to find the exact numerical relation between them. Here he had the advantage over Mayer, inasmuch as he could appeal to original experiment.

In this paper the object was to ascertain if the heating of a metallic wire by the Voltaic current was exactly in inverse proportion to its conducting power, and especially if the resistance to conduction was the sole cause of the heating effects. By a number of experiments he proves that this latter is the case, and likewise that 'when a current of Voltaic electricity is propagated along a metallic conductor, the heat evolved in a given time is proportional to the resistance of the conductor multiplied by the square of the electric intensity.' (p. 264.)

This important law was also discovered independently by Jacobi, Lenz, and Reiss, and, as shown at p. 64 of Tait, accords with the principle of conservation of energy.

Joule shows, also, at p. 275, "that the total Voltaic heat is proportional to the number of atoms (whether of water or zinc) concerned in generating the current;" and also that the heat evolved by the resistance to a Voltaic current is almost exactly the same as that produced by the combustion of the amount of the chemical decomposition which produced it.

In a paper published in February, 1842 (*Phil. Mag.*, III, vol. xx, p. 98), he proves by experiment that the quantities of heat which are evolved by the combustion of the equivalents of bodies are proportional to the intensities of their affinities for oxygen. (p. 111.)

§ 27. These data lay the foundation for the next paper, in which he was led step by step to discover the "mechanical value of heat," as he terms it, corresponding to the expression *equivalent*, first used by Mayer, and now universally adopted. The paper in question is entitled "On the Calorific

Effects of Magneto-electricity and on the Mechanical Value of Heat." (Read before the British Association at Cork on August 21st, 1843, and published in *Phil. Mag.*, III, vol. xxiii, p. 263.)

He first notices that if we consider heat as a vibration there is no reason why it should not be induced by an action of a simply mechanical character, such as the revolution of a coil of wire before the poles of a permanent magnet. But still it had not been decisively ascertained "whether the heat observed was *generated*, or merely *transferred from the coils* in which the magneto-electricity was induced, the coils themselves becoming cold." Now, he had already shown "the heat evolved by the Voltaic battery is definite for the chemical changes taking place at the same time; and that the heat rendered 'latent' in the electrolysis of water is at the expense of the heat which would otherwise have been evolved in a free state by the circuit—facts which, among others, seem to prove that *arrangement* only, not *generation*, of heat takes place in the Voltaic battery." (p. 263.)

He, therefore, set to work to clear up this point by experiment. His mode was by causing a small electro-magnet to revolve between the poles of a powerful electro-magnet.

A similar experiment is now shown in a striking manner by Foucault and Tyndall. The latter describes it at p. 33 under the singular title of "Friction against pure Space," and states that the *rationale* of the resistance to motion is not yet fully understood, but the fact is certain, and the resistance is sufficient for the development of heat. Joule's experiments are complicated and difficult to describe without seeing the apparatus or drawings of it. So the reader had better refer to the more accessible work of Tyndall, where a plate is given, and the main point, viz. the evolution of heat, is illustrated.

But, besides that, Joule's experiments display an extraordinary amount of ingenuity and labour, for the revolving body is at one time an electro-magnet, at another a bar of iron, and at another the same coated with copper. The driving force was also varied, being at one time the hand, at another a separate electro-magnetic battery. After a long series of experiments embodied in accurate tables, he says, "Having proved that heat is generated by the magneto-electrical machine, and that by means of the inductive power of magnetism we can destroy or increase at pleasure the heat due to chemical changes, it became an object of great interest to inquire whether a constant ratio existed between it and the mechanical power gained or lost," p. 435. Or, as he says in another place, subsequently,* "Whenever a current of electricity was generated by a magneto-electrical machine, the quantity of heat evolved by that current had a constant relation to the power required to turn the machine; and, on the other hand, that whenever an engine was worked by a Voltaic battery, the power developed was at the expense of the calorific power of the battery for a given consumption of zinc, the mechanical effect having a fixed relation to the heat lost in the Voltaic current."

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* *Phil. Mag.*, IV, vol. xiv, p. 2111, Sept., 1857.

§ 28. To measure this relation he ascertained the exact force required to turn the revolving electro-magnet by the falling power of known weights, and this done he had in his hands the means of solving this problem. For example, in the first experiment, "to maintain the velocity of 600 revolutions per minute, 5 lbs. 3 oz. had to be placed in each scale; but when the battery was thrown out of communication with the electro-magnet, and the motion was opposed solely by friction and the resistance of the air, only 2 lbs. 13 oz. were required for the same purpose. The difference, 2 lbs. 6 oz., represents the force spent during the connection of the battery with the electro-magnet in overcoming magnetic attractions and repulsions."

With this datum, along with others already in his possession, he was enabled to calculate the amount of heat corresponding to the surplus mechanical power. After the necessary corrections for the specific heat of the glass, the water, and the iron, he found the figure of 896 foot-pounds for 1° of Fahr. In another experiment in which the difference of falling power amounted to 3 lbs. 12 oz., the total heat evolved amounted to 9°·92. But the heat due to the chemical reaction alone, when the revolving electro-magnet was stationary, was only 5°·38, the difference, viz. 4°·54, therefore, belonged to the mechanical force due to the above falling power. By reduction, this gives 910 foot-pounds as the equivalent of heat.

The experiments in this department amounted to thirteen, and the mean result is as follows:

"The quantity of heat capable of increasing the temperature of a pound of water by one degree of Fahrenheit's scale is equal to, and may be converted into, a mechanical force capable of raising 838 lbs. to the perpendicular height of one foot." (p. 441.)

He terminates the paper by pointing out two of the practical conclusions which may be drawn from "the convertibility of heat and mechanical power into one another;" these are, that the quantity of work that corresponds to the combustion of a given quantity of fuel can now be calculated; and according to known data the best steam-engines do not utilise more than one tenth part of the heat; and also that electro-magnetic machines cannot, in an economical point of view, supersede steam.

§ 29. He adds a postscript which contains some very interesting matter.

> "We shall be obliged, after all, to admit that Count Rumford was right in attributing the heat evolved by boring cannon to friction and not (in any considerable degree) to any change in the capacity of the metal. I have myself proved experimentally that *heat is evolved by the passage of water through iron tubes.* My apparatus consisted of a piston perforated by a number of small holes working in a cylindrical jar containing 7 lbs. of water. I thus obtained 1° of heat per lb. of water from a mechanical force capable of raising 770 lbs. to the height of one foot—a result which will be allowed to be strongly confirmative of our previous deductions. I shall lose no time in repeating and extending these experiments, being satisfied that the grand agents of nature are by the Creator's fiat indestructible; and that, wherever mechanical force is expended an exact equivalent heat is always obtained." (p. 442.)

It is remarkable how near this result of the first fluid-friction experiment approximated to his final determination. But he was himself very far from being aware of its great accuracy; on the contrary, he continued to adhere to the higher estimate for several years.*

We perceive now that the grand general bearing of the subject has begun to dawn upon his mind. He passes at once to the science of physiology, and noticing that Haller attempted to account for the portion of animal heat that Crawford's hypothesis left unexplained, by the friction of the blood in the blood-vessels, he observes:—

> "It is unquestionable that heat is produced by such friction, but it must be understood that the mechanical force expended in the friction is a part of the force of affinity which causes the venous blood to unite with oxygen, so that the whole heat of the system must still be referred to the chemical changes. But if the animal was engaged in turning a piece of machinery, or in ascending a mountain, I apprehend that in proportion to the muscular effort put forth for the purpose, a *diminution* of the

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* Nevertheless, Mr. Tait, with his usual injustice, contrasts this as Joule's first result with Mayer's of 668; the truth being that Joule's own average was 838, that is to say, 8½ per cent. above, while Mayer's is 14 per cent. below the true figure.

heat evolved in the system by a given chemical action would be experienced."

It is very interesting that a similar train of thought occurred to Mayer at the very first, when he noticed the red colour of the venous blood at Batavia, as may be seen at § 1, where the total force of both heat and work are referred to oxidation. With regard to the loss of heat on ascending a mountain, it is certainly a very remarkable example of correct scientific deduction in opposition to common observation. We all know the effect of exercise in increasing the activity of the circulation and respiration, and all those functions comprising the *opus vitale* resulting in increase of animal heat. This under ordinary exertion, and even the ascent of a moderate elevation, especially when *not* fasting, more than counterbalances the loss of force given out as work. But, according to accurate experiments during an ascent of Mont Blanc in 1869, it was found by M. Lortet that his temperature, from being 36° Cent. at Chamounix descended gradually as he mounted up, until at the summit it became as low as 32°; when in a state of immobility, however, it remained at 36°, as at Chamounix. During the muscular efforts necessary for climbing the temperature may descend by 4°, or even 6° Cent., which is an enormous descent, but after a few minutes' rest it regains nearly its normal point.*

Joule concludes with a remark which has not been fully appreciated yet, viz. that the heat evolved in chemical action is not caused by the attraction of affinity, but it is rather the mechanical force expended by the atoms in falling towards one another which determines the quantity of heat evolved.

§ 30. His next paper appeared in 1845 (*Phil. Mag.*, III, vol. xxvi, p. 369), "On the Changes of Temperature produced by the Rarefaction and Condensation of Air." We have here the description of the experiments whose results have been already given at § 14.

From the compression experiments he obtained 823 and 795 foot-pounds. These, he says, were so near the 838 lbs. of the

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* *Phil. Mag.*, vol. xxviii, p. 472.

1843 paper, in which "latent heat" could not be suspected, that he is confirmed in the theory "that the heat was the manifestation in another form of the force expended in the act of condensation." (page 375.)

From the cold produced in the expansion experiments, he deduces the equivalents 814, 820, and 760 foot-pounds. His only general remark in this paper is that the phenomena herein described are incompatible with the material theory of heat.

§ 31. His next paper was in September, 1845 (*Phil. Mag.*, III, vol. xxvii). Here he states that he had abandoned the electromagnetic method, and continued the experiments with fluid friction mentioned at the end of his paper in 1843. The plan he now adopts is that of a horizontal paddle working in a cylinder. The result of the present series of experiments is 890 foot-pounds. But he now adopts as the equivalent the mean of this and his former results, viz. 817 foot-pounds (p. 206). Here he remarks that waterfalls must generate heat, and calculates that the water at the foot of Niagara must have gained one fifth of a degree. Here also appears for the first time the remarkable calculation of the *absolute zero* of heat.

"Assuming that the expansion of elastic fluids on the removal of pressure is owing to the centrifugal force of revolving atmospheres of electricity, we can easily estimate the absolute quantity of heat in matter. For in an elastic fluid the pressure will be proportional to the square of the velocity of the revolving atmospheres; and the *vis viva* of the atmospheres will also be proportional to the square of their velocity, consequently the pressure will be proportional to the *vis viva*. Now the ratio of the pressures of elastic fluids, at the temperatures of 32° and 33°, is 480 to 481, consequently the zero of temperature must be 480° below the freezing point of water." (*Phil. Mag.*, III, vol. xxvii, p. 207.)

Afterwards, as we shall find, he abandons the hypothesis of electric atmospheres, but applies these calculations to the motions of the molecules themselves, and gives their actual velocity.

§ 32. In June, 1846 (*Phil. Mag.*, III, vol. xxviii), we find this paper, "Experiments and Observations on the Mechanical Powers of Electro-magnetism, Steam, and Horses," by Scoresby and Joule. The greater part of this paper is merely technical, and consists of experiments confirming Joule's previous estimates of the working powers of electro-magnetic and steam engines.

"A horse, when its power is advantageously applied, is able to raise a weight of 24,000,000 lbs. to the height of one foot per day. In the same time (twenty-four hours) he will consume 12 lbs. of hay and 12 lbs. of corn. He is, therefore, able to raise 143 lbs. by the consumption of one grain of the mixed food. From our own experiments on the combustion of a mixture of hay and corn in oxygen gas, we find that each grain of food, consisting of equal parts of undried hay and corn, is able to give 0·682° to a pound of water, a quantity of heat equivalent to the raising of a weight of 557 lbs. to the height of a foot. Whence it appears that one quarter of the whole amount of *vis viva* generated by the combustion of food in the animal frame, is capable of being applied in producing a useful mechanical effect, the remaining three quarters being required in order to keep up the animal heat, &c."

This is the only general remark, and it certainly presents a great contrast to the stage at which Mayer had by this time arrived, for *The Organic Motion* was published the year before. Joule had, however, as yet not heard of Mayer.

§ 33. "On the Mechanical Equivalent of Heat, as determined by the Heat evolved by the Friction of Fluids," by J. P. Joule, September, 1847 (*Phil. Mag.*, III, vol. xxxi, p. 173). The apparatus used was, as before, a paddle moving in a drum filled with water. He obtains now 781·5 foot-pounds. Then reflecting that, as sperm oil is the liquid used by engineers as the best for diminishing the friction of machinery, it would be well calculated to afford a decisive proof of the principle contended for, he used it in a series of experiments, having first accurately determined its specific heat. The result was almost exactly the same as with water, being 782 foot-pounds. The mean of these, viz. 781·8, is the equivalent he now adopts.

Here we notice for the first time his use of Mayer's term, the *mechanical equivalent of heat*.

§ 34. We now come to his final paper on the subject, which was read before the Royal Society June 21st, 1849, and published in the *Phil. Trans.*, 1850, p. 61. Here he gives a short review of what has been done by Rumford, Davy, Dulong, and Faraday in proof of the immateriality of heat. In particular he notices the law of Dulong,* mentioned at § 12, as of the "utmost importance in the development of the theory of heat, inasmuch as it proves that the calorific effect is, under certain conditions, proportional to the force extended." Then he says, mentioning Mayer for the first time, "In 1834 Dr. Faraday demonstrated the 'identity of the chemical and electrical forces.' This law, along with others subsequently discovered by that great man, showing the relations which subsist between magnetism, electricity, and light, have enabled him to advance the idea that the so-called imponderable bodies are merely the exponents of different forms of force. Mr. Grove and M. Mayer have also given their powerful advocacy to similar views." Then after a short recapitulation of his previous series of experiments with electro-magnets, he alludes as follows to his fluid-friction experiments :—"From the explanation given by Count Rumford of the heat arising from the friction of solids, one might have anticipated, as a matter of course, that the evolution of heat would also be detected in the friction of liquid and gaseous bodies. Moreover, there were many facts, such as, for instance, the warmth of the sea after a few days' stormy weather, which had long been commonly attributed to fluid friction.

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* We have already seen the importance of this as the testing clause in Mayer's demonstration, § 12.

> "Nevertheless, the scientific world, preoccupied with the hypothesis that heat is a substance, and following the deductions drawn by Pictet from experiments not sufficiently delicate, have almost unanimously denied the possibility of generating heat in that way. The first mention, as far as I am aware, of experiments in which the evolution of heat from fluid friction is asserted, was in 1842, by Mayer, who states that he has raised the temperature of water from 12° to 13° C., by agitating it, without, however, indicating the quantity of force employed,[*] or the precaution taken to secure a correct result."

After alluding seriatim to his former experiments with fluid friction, during the six years that had now elapsed, he enters on his final series, which display the accumulated accuracy and skill resulting from long experience, and worthily crown his work. A complete description with a plate [†] of the apparatus is given. He enumerates the immense variety of precautions taken to obviate possible sources of error, viz. to prevent the escape of heat; to determine the specific heat of the apparatus, even to the thermometers; to allow for the elasticity of the strings, the friction of the rollers, the vibration of the apparatus, and even the noise produced. In the friction experiments with cast-iron he says, "The force required to vibrate the string of a violoncello, so as to produce a sound which could be heard at the same distance as that arising from the friction, was estimated by me, with the concurrence of another observer, at 50 foot-pounds."

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[*] From this it appears he had not seen Mayer's *Organische Bewegung*, for there the horse-power is given, and the whole observation on the paper mill is exactly parallel to Rumford's on the boring of cannon, § 22.

[†] The description and figures may be seen in several manuals of physics and

The experimental results are summed up as follows:—

40 experiments with water, giving a mean of 772·692.
50 ,, mercury ,, 774·083.
20 ,, cast-iron ,, 774·987.

On account of certain corrections still to be allowed for he finally adopts the figures 772. He brings to an end this classical work with the following conclusions:—

"1st. The quantity of heat produced by friction of bodies, whether solid or liquid, is always proportional to the quantity of work expended. 2nd. The quantity of heat capable of increasing the temperature of a pound of water (weighed *in vacuo*, and taken at between 55° and 60) by 1° Fahr., requires for its evolution the expenditure of a mechanical force represented by the fall of 772 lbs. through the space of 1 foot."

"These experiments," says Tyndall, "rank among the most remarkable that have ever been executed in physical science. They form of themselves a strict demonstration of the dynamical theory of heat."

§ 35. These conclusions were adopted by the whole scientific world, and the doctrine became established. Mayer also in the same year, 1850, finished his series of essays with the truly philosophic treatise called *Remarks on the Mechanical Equivalent of Heat*. The subsequent experiments by Favre, Person, and Foucault, in France, and

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chemistry, viz. *Ganot's Physics*, *Fownes' Chemistry*, 10th edition, and *Miller's Chemistry*, 4th edition, vol. i, p. 243.

Icilius in Hanover, merely confirmed the results of Joule without adding to their conclusiveness.

The history of the discovery may be said to close here, for the principle being now the property of the scientific world in general, its application becomes part of the progress of the different parts of physical science, over all of which it exercises a most important influence.

§ 36. A few words remain to be said as to the reception given by the scientific world to the authors of this discovery. Since 1850 we may say the palm has been awarded by acclamation to Joule as the experimental demonstrator. Among the individual testimonies to his merit we may quote the following. J. Bertrand writes:

"Joule, faithful to the experimental method of Galileo and Newton, affirms nothing that he has not verified, and reserves for conclusions the absolute theorems and the general laws which are principles for Mayer. Joule measures with as much patience as talent; he estimates the amount of the physical forces in the course of their metamorphosis, and without affirming that they ought to remain constant, he gradually arrives at the proof that they are so. The conclusions of his researches cannot be contested, and more than one mind devoted to rigorous exactness, which the brilliant assertions of Mayer were unable to convince, have been obliged to regard the precise demonstrations of Joule as indispensable" (p. 674). Tait says, at p. 66, "The experimental foundation of the principle in its generality, and the earliest suggestion of some of its most important applications, belong unquestionably to Joule. Trained to accurate experiment in the school of Dalton, the pupil has not only immortalised himself, but has added to the fame of his master." Verdet says, "You will not refuse to see in the now-classic work of Joule, the experimental proof of the exactitude of the new principles" (*Phil. Mag.*, May, 1863). Tyndall thus speaks of him: "The man who through long years, without encouragement, and in the face of difficulties which might well be deemed insurmountable, could work with such unswerving steadfastness of purpose to so triumphant an issue, is safe from depreciation. And it is not the experiments alone, but the spirit which they incorporate, and the applications which their author made of them, that entitle Mr. Joule to a place in the foremost rank of physical philosophers" (*Phil. Mag.*, May, 1863). Further testimonies might be quoted, but on this point there is no dissentient voice.

§ 37. It is otherwise with Mayer, for, owing to the mode of publication of his chief essays, and other reasons, his labours were overlooked, and due credit withheld from him, especially in this country. Time, however, and the intrinsic excellence of his works, have obtained justice for him, as the following, among many testimonies that might be cited, sufficiently show. One of the first to recognise the true position of Mayer was Helmholtz, who, in 1854 (*Phil. Mag.*, XI, p. 489), writes:

"The first man who correctly perceived and rightly enunciated the general law of nature, which we are here considering, was a German physician, J. R. Mayer, of Heilbronn, in the year 1842. I believe that Mayer must be considered as a man who independently, and for himself, discovered this thought, which has produced the grandest recent advance of natural science" (Tait, vii). The French historian of this subject, Verdet, also gives him the position of priority in respect to the whole doctrine, and likewise to the discovery of the mechanical equivalent of heat by the method of the two specific heats, which he says is perfectly exact in principle. And in respect to his views of solar influence on organic life, he thus speaks:—"These ideas, introduced for the first time into science, in 1845, by J. R. Mayer, gave an impulse to the progress of general physiology, assuredly equal to the progress which resulted towards the end of last century from the discoveries of Lavoisier and Sennebier on respiration." Bertrand writes:—"J. R. Mayer's book will remain, I venture to affirm, one of the most important productions of our epoch. I have given principle and object; but the developments of them are as ingenious as profound, and place without contradiction its author among the most illustrious discoverers" (p. 665). "Mayer went further in affirming, amid all these devious paths, the necessary and absolute equivalence among the different forms of the same energy, or, according to his language, between the different manifestations of the same force." (p. 663.)

Then, after analysing "The Celestial Dynamics" with expressions of great admiration, he concludes thus:—"The preceding pages summarise very incompletely the beautiful work of Mayer. It is to him, I repeat, that appertains the honour of having affirmed and developed clearly the *ensemble* of those principles which are now accepted by all." (J. Bertrand, p. 671.)

Professor Fick says (*Naturkräfte*, p. 3), "It is to a German physician, J. R. Mayer, that incontrovertibly belongs the merit of having first communicated in a distinct form the fundamental idea of this doctrine, and accompanied with precise quantitative determination. This was in May, 1842. With this date begins a new epoch in natural science."

But it is to Tyndall that in this country we are indebted for a knowledge of the great merits of Mayer. "Without external stimulus, and pursuing his profession as town physician in Heilbronn, this man was the first to raise the conception of the interaction of natural forces to clearness in his own mind. And yet he is scarcely ever heard of in scientific lectures, and even to scientific men his merits are but partially known. When we consider the circumstances of Mayer's life, and the period at which he wrote, we cannot fail to be struck with astonishment at what he has accomplished. Here was a man of genius working in silence, animated solely by a love of his subject, and arriving at the most important results, sometimes in advance of those whose lives were entirely devoted to natural philosophy."

In deprecation of any attempt to place either of these men in the position of exclusive claimant of the honour of the discovery, Tyndall says further :—" I would here state, once for all, that I should not think of putting Mayer above Joule, or Joule above Mayer. In the firmament of science they are in my opinion a double star, the light of each being in a certain sense complementary to that of the other." (*Phil. Mag.*, July, 1863.)

" In reference to their comparative merits, I would say that, as seer and generaliser, Mayer in my opinion stands first—as experimental philosopher, Joule."

This, I think, may be accepted as the general opinion of competent judges.*

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* The merits of Mayer were for some time not recognised even in the land of his birth, and in England they were for long entirely ignored, and are scarcely yet generally known. Except by Carpenter, who gives it as his opinion that " Mayer was the first who appreciated the full practical bearing of the doctrine, and assumed it broadly in all its generality ;" and by Lionel Beale, who also was early aware of his merits, he receives little justice from physiologists, and is not even mentioned in several of the compendiums in the hands of our students. For example, in Fownes's tenth edition the whole credit of the discovery is given to Joule, and Mayer's name (wrongly spelt) occurs only once, as follows :—" Meyer showed that the temperature of water may be raised 22 or 23 F. by agitating it." C. Brooke does not mention his name at all in the chapter on the subject, attributing the whole discovery to others (*Nat. Phil.*, § 1344 to 8). In Bence Jones's *Matter and Force* the name of Mayer is not mentioned, the whole credit of the discovery being given to Joule. In France, for long, it did not fare much better with him, for in Laugel's paper in the *Revue des deux Mondes*, September, 1858, called " L'esprit de la Physíque Moderne," which treats of the total change of the aspect of the physical sciences being brought about by the conservation of force doctrine, Mayer's name is not once mentioned ! The discovery of the M. E. H. is given to Joule, confirmed by Favre and Person. But these instances of neglect are nothing to the position of actual hostility to his claims taken up by Professor Tait, who has shown a heat of partisan-feeling very difficult to understand. After giving some

Finally, he has recently received one of the highest honours that can fall to the lot of a scientific man in being

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examples in which the credit of British discoveries has been appropriated by foreigners, he seems determined that it shall not be so in this case, and the best way to prevent it, he thinks, is to take up the position of a partizan, which he avows in these words :—

"It is possible that circumstances may have led me to regard the question from a somewhat too British point of view. But, even supposing this to be the case, it appears to me that unless contemporary history be written with some little partiality, it will be impossible for the future historian to compile from the works of the present day a complete and unbiassed statement. Are not both judge and jury greatly assisted to a correct verdict by the avowedly partial statements of rival pleaders? If not, where is the use of counsel?" *Preface*, p. v.

Be this doctrine right or wrong morally, the results are unquestionably unfortunate; and in every mention he makes of Mayer we hear nothing but the advocate speaking, while the historian and scientific judge is silent. The consequence is a distorted or totally false representation of Mayer's claims in almost every instance. This has brought a remonstrance from Helmholtz which it is pleasing to see Tait has the candour and manliness to print in the preface to his present work; and it is to be hoped that it will induce him to change his views at some future time, though he has not done much in that way in the present edition. He is ostensibly pleading the cause of this country in claiming the exclusive merit of the discovery for Joule; but I hope and believe that though the British people love the glory of their great men much, they love truth and justice more. Likewise Joule, who is a great man, and whose name for future ages will stand above those of Torricelli and Dalton, must disdain flattery at the expense of truth and justice; and no doubt he must reciprocate the sentiments with which Mayer speaks of him in the last essay of his volume:

"In fact this law and its numerical expression, the mechanical equivalent of heat, was made known almost simultaneously in Germany and in England. Starting from the fact that the amount of chemical as well as of galvanic effect depends solely on the amount of material consumed, the celebrated English physicist, Joule, was led to the axiom that the phenomena of heat and motion rest essentially on the same principle, or, as expressed by him as well as myself, that heat and movement are mutually convertible. The independent discovery of this natural law cannot be contested to this man of science; and in addition, he has earned the merit of numerous and important services in the establishment and development of the same." Some of these he alludes to, and more especially the well-known friction experiments. (Mayer, p. 289.)

Several examples of the evils of the advocate style of writing history have already been given, so I will merely notice here Tait's final summary of the successive stages of the development of the science of energy. These are nine, of which Newton, Davy, Rumford, Fourier, and Carnot have each one. For the four last nothing is to be said against their claims, except that they did elected Corresponding Member of the Paris Academy of Sciences, in place of Matteucci, deceased.

§ 38. To sum up, it may be observed that the history naturally divides itself into two distinct periods, viz. those before and after the epoch 1839 to 1843.

Since the time of Newton and Leibnitz the laws of ordinary dynamics have been settled; but between them and the so-called imponderable forces there existed a great gulf, and until this was bridged over by a common measure of value no general dynamic theory of all the forces was possible, and all attempts at a universal theory remained mere speculations, and barren of results to the progress of science. When this was discovered, however, many of them became valid, and their authors must be received as the precursors of the modern doctrine. In the first period, as precursors of the general theory of energy, may be named Descartes, Huyghens, the Bernouillis (Joh. and D.), Leibnitz, Heinrich and Mohr,* Faraday, and others; while those

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not become valid till after the discovery of the M. E. H. But as respects Newton, the claim put forward by Tait in virtue of a passage fished up by him from a scholium to the third law of motion has already been the subject of controversy, and Dr. Akin has maintained (*Phil. Mag.*, 1864) that it really bears no such interpretation. And, besides, he gives passages from the optics where Newton distinctly teaches the absolute annihilation of force; and, further, that he taught what is still in all the school books, that motion is absolutely lost by friction, &c. Coming down to the M. E. H. period, the seventh stage is the discovery of it by Joule, while the name of Mayer is not mentioned at all. But to make up for it we have that of W. Thomson put in three times; he has the sixth and ninth all to himself, and is admitted with Rankine and Clausius into the eighth. This provokes one to ask, What for? and we see that the ninth is given to Thomson for his theory of dissipation of energy! Certainly with the fact of radiation of heat known to all the world, when once the transformation of the forces was discovered the dissipation of energy became such an obvious corollary that one would scarcely have expected to see it put into such a position. The truth is, in all Tait's books there is a constant effort to thrust Thomson and Joule into prominence; and Thomson may well say, save us from our friends! Fortunately, the reputation of Joule is too high to be damaged by the efforts of even such an injudicious friend as Tait.

* Professor Bohn and Dr. Akin have given interesting papers in vol. xxviii, IV, of the *Phil. Mag.*, on these anticipations. In particular the latter gives a quotation from an article by Mohr, of Coblentz, in 1837, where he says the force of the arm in turning the electro-magnetic machine reappears as heat, light.

connected with the dynamic theory of the imponderable forces were Bacon, Locke, Rumford, Davy, Fresnel, Young. At the epoch alluded to, the idea of an equivalent relation between the mechanical and molecular forces was seized upon almost simultaneously by four men. The claims of Séguin and Colding to honorable mention have been considered. But the discovery of the mechanical equivalent of heat rested with Mayer and Joule, and it was owing to the experimental proofs of the latter that the physicists received it as established.

This discovery is in itself the foundation of the subsequent progress of the science, and whoever made it might be regarded as the beginner of a new era, even if he did nothing towards the application of it. Now, Joule did this to the satisfaction of all, and we may say with truth, if no one else had done anything towards the discovery, science in general would have been as far advanced as it is now.

But does this give the sole credit to Joule? By no means, for the same may be said of Mayer; for, without Joule, the same impulse would have been given to science though somewhat later, and some of the English school who are now eminent in this field might not have obtained their distinction. In fact, Joule can claim no more than bare equality in the discovery of the mechanical equivalent of heat; for Mayer certainly had the priority in its discovery. He looked for it with intention, and found it, with full consciousness of its importance, by a method now proved perfect. And, in addition, he followed it up to its full development in a way then unequalled, and which, probably, will never be surpassed by any single man. The labourers in this field since that epoch may be classed thus. In the front, in a rank occupied by himself alone, stands Mayer, the man of genius, the discoverer, and the generaliser. Next (in the general question of energy) come Helmholtz, Rankine, Clausius, Joule, and Thomson. After them come

chemical affinity, and magnetism. This anticipates a passage from Joule. And "what . . . produces a force must itself be a force. What . . . counteracts a force must itself be a force. Heat appears in innumerable cases as a force." These expressions are in various places paralleled in Mayer's book.

Faraday, Tyndall, Grove, Maxwell, Waterston, Carpenter, Regnault, Frankland, Fick, and many others whom it is no disparagement not to name, as this has no pretensions to being a complete history.[*]

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[*] *Note.*—I had nearly finished the foregoing pages from data furnished by the *Phil. Mag.* up to 1860, and the standard works quoted, when I met with a remarkable controversy on the claims of Mayer, in the *Phil. Mag.* in 1863 and 1864. To us, who are not physicists, this correspondence is extremely astonishing, and contains an element of fun in it that one would scarcely expect in the grave pages of a journal devoted to the exact sciences. We are often given to understand that in the physical sciences any assertion can be at once submitted to mathematical calculation or to experiment, and there is no room for those contests of opinion, far less the unseemly squabbles which disfigure medical periodicals. A glance at this controversy dissipates all such Arcadian notions, and we understand at once Sir H. Davy's smile when the young Faraday desired so ardently to enter the ranks of science because of the mental elevation it induced. Instead of the measured language of the calm and passionless physicists, we have here a vigour and sharpness of style that make us, of the house of medicine, feel quite at home; which, in fact, would equal that of politicians, or even rival the bitterness of the theologians themselves! The occasion was a lecture on 'Force,' by Tyndall, in 1862, wherein due place was given to Mayer. This attracted the notice of Sir William Thomson and Professor Tait, who had taken up a strong opinion on the exclusive claims of Joule, which they thought it their duty to make known, and even to censure Tyndall for his want of patriotism. But. instead of doing so in a scientific organ, they proceed to whisper it into the ears of the—Tait boasts—120,000 readers of *Good Words*. After a time this also comes to the ears of Tyndall, who in March, 1863, writes in the *Phil. Mag.*, expressing his surprise at the publication chosen for such a subject, pointing out his ground for his statements, and mildly requesting to be explicitly informed wherein he had misstated any facts. Now begins a kind of triangular duel, of a very singular kind, and in which the buttons are off the foils with a vengeance. Next month (April) Tait writes, for Thomson and himself, a reply in a style of cool insolence perfectly amazing. After sneering at Tyndall's taste for the re-habilitation of wronged foreigners, he actually informs him of all the assertions they had been making, as if they were undoubted facts, in the manner of a schoolmaster instructing little boys. This to Tyndall, who might have been thought as well as, or better informed on the subject than any man in England! Flesh and blood could not stand this. So Tyndall boils over with indignation and answers, in a letter couched in words that remind us of the days of Milton and Salmasius. He addresses his letter exclu-

sively to W. Thomson, "the elder and more famous man, whose behaviour is the only one of interest to the world," and telling Tait to have the grace and modesty to stand aside till they settle the affair. Then he goes through the whole question, and bit by bit brings out the facts of the matter, which have been given almost completely above, and it is indeed satisfactory to me that the sufficiency of the plan adopted, viz. explaining the matter by following the footsteps of the discoverers, is corroborated, and I find but very little to add, and only a few sentences have been inserted here and there since I read this controversy.

He takes Thomson to task for his want of acknowledgment of the priority of Mayer in several instances in which he himself is concerned, and charitably hopes that it was rather from ignorance of Mayer's works than plagiarism. He proves with ruthless precision of dates and quotations that it must have been the one or the other. In particular he refers to the meteoric theory of solar heat, and to the loss of the earth's rotation by tidal friction, on which subjects he had silently watched Thomson's conduct with pain for several years, and he still invites him to have the manliness to retract. Thomson, however, remains obdurate; he has taken many cities, but has not yet obtained the victory over himself. For, in 1866, we find him writing a paper *On the Tidal Retardation of the Earth's Rotation,* wherein Mayer's name is not even mentioned. The magnanimity of Tyndall appears now, for, after all this, in the later editions of his book on heat, no word in dispraise of Thomson appears; and, in fact, it is chiefly through Tyndall that Thomson is favorably known to the general public, for his own works are extremely abstruse. To return to the battle of 1863, which still rages, though now the parties do not write *to* each other, but *at* each other in letters to Sir David Brewster. In June Thomson has a short note evidently written in the sulks; he stands upon his dignity, and declares he will not be spoken to in that way; says he won't answer, and he doesn't, but pushing forward his henchman Tait, he backs out of the fray, and is no more seen. On comes Tait with a dogged courage worthy of a better cause. His tone is now much more moderate, and he is compelled to admit certain facts, though he argues against the interpretation put upon them, his mind being evidently warped by the position of an advocate, then taken up. Again, in July, Tyndall appears setting him right on some minor matters. At length the din of the contending heroes is heard in Olympus, and now the demigods step down into the arena. We have a letter from Mayer himself. Here the man of genius shows his wonted amiability and candour; he thanks Tyndall for his defence of his rights, "which makes all the deeper impression, from the fact that for many years I have been forced to habituate myself to a precisely opposite treatment." But he speaks with respect and cordiality of Joule, whom, he says, "I have never regarded as an antagonist, but, as you truly expressed it, have always considered him to be my esteemed and renowned fellow-labourer in the same domains of thought."

After this we have two letters from Joule, which do not make so pleasing an impression, as he is disposed to claim for the experimental demonstrator the exclusive merit of the discovery, though he acknowledges the intellectual merits of Mayer. His letters are, in fact, chiefly a continuation of the subject already discussed in the 'Comptes Rendus,' and are mostly made up of extracts in French. It is here, also, he attributes that calculation of the M. E. H. to Séguin which has been noticed at § 20. Tait, again, has two papers in which, while compelled to give in as to the facts, he adheres to the line first taken, and still continued in his book in 1868, viz., that of the advocate, by spreading the intellectual merit among such a host of competitors, from Newton downwards, that he contrives to give Mayer a very subordinate place, while the whole merit as the discoverer is given to Joule. Tait now adds to his other misdemeanours the unpardonable crime of becoming dull from mere iteration. The reference to Newton calls up Dr. Akin and Prof. Bohn, of Giessen, who correct him on some points. Ultimately Tyndall, in July, 1864, writes a complete and exhaustive paper on the whole subject entitled "Notes on Scientific History," which clears up any points left doubtful, and he concludes by stating that he has received the assurance of several competent judges both at home and abroad of the justice of his stand-point, and highly approving of his conduct. We rise from the perusal of this controversy with the conviction that the votaries of the exact sciences are not the passionless beings we have been led to suppose, but are very much as other men are. We receive if possible a more exalted idea of the genius and character of Mayer, and sympathise more fully with him in the great misfortunes which so long kept him from his well-earned fame. We are a little disappointed to find Joule still stickling for the exclusive claim of the experimentalist to the discovery, though we are glad to see that, like a great man as he is, he candidly acknowledges a mistake. We rejoice in the pluck and spirit of Tyndall, who stands forward like a true-hearted Englishman to fight the battle of truth and justice.

CHAPTER II.

NATURE AND DEFINITION OF FORCE.

§ 39. In the foregoing chapter the history of the discovery of the mechanical equivalent of heat was traced. The effect of this discovery upon the progress of science may be described as incalculable. At once the dynamic theory of heat was established, and that of all the other so-called imponderables was almost taken for granted. The whole universe appeared now to arrange itself into two great categories, viz. of matter and its motions—matter with its properties the object of chemistry, and force or motion that of physics, which last is brought under the laws of mechanics. This apparent simplicity, however, is not yet attained, for we find ourselves immediately confronted with a new claimant for our notice in the person of "energy." What is this energy? Are we to be again plagued with the "essences" of the ancients, which were supposed to give the qualities and forces of matter by combination with it; or is it merely a new name for force invented to obviate a real difficulty which we now encounter? That force in action is capable of being explained by the movement of either masses or molecules of matter we can readily enough understand, but there is another equally important condition in which it may exist, when it is said to be stored up; and the body is then said to possess potential force or energy. How is this to be explained? how can we conceive motion that is not motion—or, motion in a state of rest? To arrive at a clear understanding of this subject, it will be necessary to go at length into the general question of the nature and definition of force.

§ 40. In the *Imperial Dictionary* we find the following definition of the word "Force:"

> I. Strength, active power; vigour; power; might; energy that may be exerted.
>
> II. Momentum; the quantity of power produced by motion or the action of one body on another; as the *force* of a cannon ball.
>
> III. Strength, energy, as *the force* of the mind, will, or understanding, the *force* of an argument.

Even in scientific works, where greater precision is required, there still prevails a great want of uniformity in the use of the word, which is the source of much confusion. Thus the term force is used in three different senses:—1st, as the cause of motion; 2nd, as actual motion; 3rd, as the result of motion. For example, the chemical actions which start a cannon ball, the motion of the ball, and finally the power or strength of the blow, have all been called force. To escape the confusion, some authors think it sufficient to say that here the powder has force, and the shot has energy. The force of the powder before ignition is here also called potential, *i.e.* capable of being called into activity. But, unfortunately, the same author immediately uses the same word potential in conjunction with "energy;" so that we at once lose the greater precision which we seemed to have gained. As the subject is intricate, it may be best to enter somewhat minutely into an examination of the definitions given by the leading authorities, stated as far as possible in their own words. The ordinary definition of force in dynamics is, "Force is any cause that changes or tends to change a body's state of rest or motion." (Goodwin's *Mathematics*, p. 203.)

The definition, as quoted by Mayer, p. 259, is, "Force is whatever produces or tends to produce, changes, or tends to change, motion."

This, as Mayer remarks, is redundant, and may be reduced to the simple definition "that which produces motion." How far this is sufficient is now the question.

We cannot do better than enter on the subject under the guidance of the clear-sighted Mayer. He appositely remarks, "The question is not what force is in its essence, but to what the word force is to be applied."[*] In the same sense Tait says, "Till we know what the ultimate nature of matter is, it will be premature to speculate as to the ultimate nature of force; though we have reason to believe that it depends upon the immediate action of highly attenuated matter diffused throughout space." The latter clause is objected to by Sir J. Herschel,[†] and in fact ridiculed, "as resolving the entire assemblage of natural phenomena into the mere knocking about of an inconceivable number of inconceivably minute billiard balls." These extracts illustrate the above remark of Mayer, for it is evident the question is already diverted to the origin and physical nature of force.

§ 41. Let us, therefore, leave on one side these questions, and follow Mayer:

> "The word force is used in two different senses in the higher or scientific mechanics, viz.:
>
> "I. By it is understood any pressure or pull, any effort of an inert body to change its state of rest or motion, and if this effort be regarded by itself, and independently of the result, it is called 'pressure force,' 'pulling force,' or shortly 'force,' also, in contradistinction to the following sense—'dead force.'
>
> "II. In another sense, what is called 'force,' is the product of the pressure into the space through which it acts, or also the —whole or half—product of the mass into the square of the velocity. For it is necessary to the production of every real motion, that the body in question should actually traverse a certain space under a pressure, and in the direction of it, and now the magnitude proportional to the pressure and the space passed through is also called 'force;' but for distinction from the mere pressure, which alone never produces actual movement, it is called '*vis viva* of motion,' or 'moving force." (p. 258.)

He points out that the first of these senses is the one adopted by Newton, and the second by Leibnitz. To avoid the confusion inseparable from an ambiguous term, he gives reasons why we should either abandon the use of the word force altogether, or confine it strictly to one of these two senses, and if the word is retained, he has no hesitation in giving his verdict for the second sense. For he says that, strictly speaking, the first is only applicable to statics, where the pressure is constant and the space null, and to the higher departments of mechanics; and that it is undesirable to define a fundamental conception in a way which excludes it from the elementary stage of a science. Having decided in favour of the second, he shows that it is in conformity with the laws of thought and the usage of language to connect the production of all motion with the expenditure of force, and thus arrives at the following definition:

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[*] *Was eine Kraft für ein Ding ist, sondern welches Ding wir Kraft nennen wollen*, p. 271.

[†] "On the Origin of Force," *Fortnightly Review*, vol. i, p. 435.

§ 42. "*Force is that which is expended in the production of motion; and this which is expended is, as cause of the effect, equal to the motion produced.*" (p. 265.) This will be found to be the most intelligible in a practical point of view, and to render us most assistance in the conception of the true nature of the transformations of the physical forces.

§ 43. Helmholtz divides force into Spannkraft and lebendige Kraft. These terms have been generally adopted by physiologists in Germany. In English they have been rendered by the terms tensive and active force, or simply tension and *vis viva*, the latter being used instead of the literal English rendering *living force*, in order to avoid the ambiguity which the use of this last expression might occasion in physiology.

§ 44. W. Thomson calls the Spannkraft "statical mechanical effect," and the lebendige "dynamical." He says—

> "It is convenient to divide stores of mechanical energy into two classes—statical and dynamical. A quantity of weight at a height, ready to descend and do work when wanted, an electrified body, a quantity of fuel, contain stores of mechanical energy of the *statical* kind. Masses of matter in motion, a volume of space through which undulations of light or radiant heat are passing, a body having thermal motions among its particles (if not infinitely cold) contain stores of mechanical energy of the dynamical kind." (*Ph. Mag.*, IV, iv, p. 304.)

§ 45. Rankine makes the following important contributions to the subject:—

> "In this investigation the term energy is used to comprehend

every affection of substances which constitutes, or is commensurable with a power of producing change in opposition to resistance, and includes ordinary motion and mechanical power, chemical action, heat, light, electricity, magnetism, and all other powers, known or unknown, which are convertible or commensurable with these. All conceivable forms of energy may be distinguished into two kinds—actual or sensible, and potential or latent. *Actual energy* is a measurable, transferable, and transformable affection of a substance, the presence of which causes the substance to tend to change its state in one or more respects; by the occurrence of which changes actual energy disappears, and is replaced by *potential energy*, which is measured by the amount of change in the condition of a substance, and that of the tendency or force whereby that change is produced (or what is the same thing, of the resistance overcome in producing it) taken jointly. If the change whereby potential energy has been developed be exactly reversed, then, as the potential energy disappears, the actual energy which had previously disappeared is reproduced.

"The law of conservation of energy is already known, viz. that the sum of actual and potential energies in the universe is unchangeable." (From Rankine's paper "On the General Law of the Transformation of Energy," *Phil. Mag.*, s. IV, vol. v, p. 106.)

The phrase "potential energy" is objected to by Sir J. Herschel as "unfortunate, inasmuch as it goes to substitute a truism for the announcement of a great dynamical fact." In reply to this Rankine* says, the kind of quantity meant is expressed by Newton in the 39th proposition, though only in symbols. Then Rankine continues:—"The application of the word 'force' to that kind of quantity is open to the objection that when force is taken in the sense in which Newton defines *vis motrix*, the power of performing work is not simply force, but force multiplied by space. To make such an application of the word force, therefore, would have been to designate a product by the name properly belonging to one of its factors, and would have added to the confusion which has already arisen from the ambiguous employment of that word." "About the beginning of the present century the word 'energy' had been substituted by Dr. Thos. Young for *vis viva*, to denote the capacity for performing work due to velocity; and the application of the same word had at a more recent time been extended by Sir William Thomson to the capacity of any sort for performing work. There can be no doubt that the word 'energy' is especially suited for that purpose; for, not only does the meaning to be expressed harmonise perfectly with the etymology of ἐνέργεια; but the word energy has never been used in precise scientific writings in a different sense, and thus the risk of ambiguity is avoided." (p. 89.)

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* "On the phrase Potential Energy," *Phil. Mag.*, 1867.

He (Rankine) therefore adopted it and qualified it by the adjectives, actual and potential, "to distinguish between energy of activity and energy of configuration." Thomson and Tait have lately used kinetic instead of actual. Carnot had already used the term "*force vive virtuelle*" for potential energy in its purely mechanical sense. (Rankine, loc. cit.)

§ 46. Tyndall adopts Rankine's terms potential and actual energy; but he uses also the words possible energy for the former and dynamic energy or moving force for the latter. (p. 131.)

B. Stewart and N. Lockyer in a joint paper in *Macmillan's Mag.*, Aug. 1868, adopt the terms *energy* of *actual motion* and *energy of position,* in place of actual and potential energy.

Bertrand says: "The word force, as we understand it, will always designate *an effort expressible in kilogrammes.*"—J. Bertrand, p. 583. Miller thus speaks:

> The only mode in which we can judge of the existence of a force is from the effects it produces, and of these effects that which is most universal is the power either of producing motion, of arresting it, or of altering its direction; whatever possesses this power has been looked upon as a form of force. Motion is consequently regarded as the signal of force." (p. 616.)
>
> According to Grove, "a force cannot originate otherwise than by generation from some antecedent force or forces." (p. 14.) "No force can, strictly speaking, be initial, as there must be some anterior force which produced it; we cannot create force or motion any more than we can create matter." (p. 103.)
>
> "Force without antecedent force is inconceivable any more than matter, except by a creative power." (pp. 103-4.) "These affections of matter called forces are themselves modes of motion." (p. 98.)

In reference, probably, to these last views, Sir J. Herschel remarks that the tendency now is to deny the existence of force altogether, and to maintain that, as we see nothing but the transference of motion from matter to matter, there can be no reason "to interpose an unknown agent or intermedium—force—as part of the process."

Here we must presume that Herschel is objecting to the statement that "no force can, strictly speaking, be *initial,*" for he adduces the volition of animals as a true initial force. But as he admits the general principle of conservation of force, as applicable to the bulk of the muscular action required to carry out that volition, and is sensible of the disturbance of the balance which such an origination of force would introduce, he is compelled to explain "that the actual force necessary to be originated may be no greater than is required to remove a single material molecule from its place through a space inconceivably minute."

The question becomes in this way one of such a purely speculative character, and is removed so completely out of the sphere of experiment, that we may without hesitation pass on to the illustration of Mayer's eminently practical definition of force.

§ 47. All these definitions are mere descriptions of, or modes of naming certain phenomena, and have no pretension to be any explanation. To say that a raised weight, or separated carbon and oxygen have potential energy of position is not to explain what it is that draws or pushes masses of matter together, *i. e.* gravity, or draws atoms together, *i. e.* chemical affinity. The most that has been done is the establishment of the great law that an equivalent amount of an object which we know in the form of visible motion of masses of matter must be consumed in counteracting these forces, and reappears again exactly when they are allowed to exert their action. So long as our knowledge is so circumscribed, Mayer's seems to us the best of all the definitions of force. It expresses fully and accurately all that we know with absolute certainty of the subject, and likewise gives a simple and unerring test of what we shall find a thing of paramount importance as we approach physiology, viz. the distinction between the forces and properties of matter. For nothing which cannot produce motion by its expenditure can be a force; and conversely whatever is capable of expenditure cannot be a property. The inherent properties can neither be lost, nor increased nor diminished in the smallest degree, nor can they be transferred from one particle of matter to another; whereas, it is the very essence of force that it is transferable, transformable, and capable of indefinite increase and diminution, possibly even to total extinction; and besides what one body loses of force another may gain, which cannot be said of any property. And if this last clause *seems* not to hold good in the case of some particular forms of force; if for example, we cannot take gravity from one body and give it to another, that is because we do not yet know the nature of these forces and their correlation with the other forces.

§ 48. The term *energy* is very nearly synonymous with Mayer's force, but in the usual definition there is not that stress laid on the essential point of expenditure. As Mayer argues, we must deny to mere dead pressure alone the name of force, nor even does the play of loaded scales or of a pendulum require any continued application of force, and it would go on for ever but for the friction. No force is expended. But if one scale were emptied at the top and loaded again at the bottom, the oscillation would not go on without the expenditure of fresh force. Newton recognised the distinction, when he says that weight is a *causa mathematica* and not a *causa physica.* But others have neglected that, and called weight, apart from the necessary condition of space to be traversed, a cause of motion, and thus induced the erroneous idea of the *production of motion without expenditure of force,* because by the falling of a weight none of the weight is expended, it is just as heavy as before; in fact it is heavier. Thus falling force is no exception to the rule given by the definition, for the cause of gravity is not inexhaustible, as a quality or property must needs be, but has a *maximum* which is invariable, so that when a weight reaches the earth its falling force is already exhausted.

"When," says Mayer, "a weight has fallen from an infinite height to a distance of 15 feet (Fr.) from the earth, then $\frac{1299999}{1300000}$ of the total falling force have been expended; $\frac{1}{1300000}$ of this force is still left, and it is by the expenditure of this proportionally very small force, that a proportionally small effect, the movement of mass with 30 feet (Fr.) velocity, is produced. It is clear, therefore, that the falling motion forms no exception to the proposition of the proportion existing between motion and expenditure of force. The expenditure becomes null only when a weight merely presses, and does not at the same time fall. A constant force which displays action, without suffering diminution, does not exist for the physicist." (p. 35.)

So in fact the force of gravitation may be put in the same category with elasticity, and a raised weight is like a bent spring—you get back just the force expended in raising the weight or bending the spring—no more. Therefore neither weight nor elasticity is a force in the sense of possessing energy *per se.*

§ 49. Mayer's view of laying the stress on expenditure is confirmed also by Helmholtz, who says, "The characteristic of the physical forces is, that when they produce any change or any alteration, when they produce motion, their faculty of acting is destroyed by their action itself; that they cannot act without diminishing or losing the faculty of new action" (p. 415). This is very clear, but already the difficulty as to the distinction between dead pressure or dead pull and force has been felt by him, for he says, "The force of affinity draws together the atoms of coal and oxygen, and produces a velocity of molecular motion, *i. e.* heat; but if this velocity is removed, the force of chemical attraction between the molecules of coal and oxygen exists in the same degree as before, but its only effect is to keep together the atoms which form *Carbonic Acid;* they cannot then be separated" (p. 388). How is this? A force is consumed in its very action, but we have here a force drawing the atoms together with such strength as to excite tremendous heat and at the end it remains as strong as before! In fact, we know that it is impossible to separate the carbon and the oxygen, without communicating for the use of the separate elements exactly that amount of (molecular) velocity which came out as heat in their combination. This is parallel exactly to the falling weight which in the act of falling consumes all its falling force, but when at the bottom is as heavy as ever. What then is this "that is expended in the production of motion?" Is it something absolutely unknown and unknowable? Or can we not arrive at an idea of it by generalizing from the one thing which we certainly know is productive of motion by its expenditure, viz., motion itself? We know that one body may transfer its motion to another, and may not this afford the explanation of what happens in all the transformations of force? In other words is not force simply motion of matter either in masses, or molecules, or atoms? The tendency of physical science is to answer in the affirmative, and in fact, the dynamical theory of the forces means neither more nor less than this. Already heat, light, magnetism, and electricity, are with more or less success referred to molecular motions and reduced to calculation on the ordinary principles of dynamics.

§ 50. The earnest student is constantly perplexed and baffled by the way in which the difficulties of the subject are passed over in most of the semipopular treatises on this subject. We hear over and over again, that the sum of the potential and actual energies of the universe forms a constant quantity, or that the great thing is to recognise energy in its two forms, viz., that of position, and that of actual motion, and then we are supposed to comprehend the whole matter, whereas our difficulties may almost be said to be only beginning. The truth is, whatever abstract conceptions we may form of it, force is merely an affection of matter and is inconceivable as separated from matter, and can only exist in reality as some individual force. Neither can any one force be viewed as the central force which may by its transformations produce all the rest; but each force is capable, mediately or immediately, of producing any other. Now comes the difficulty: we know some forces only as motion, such as that of a moving body, heat, light, &c. On the other hand, the forces on which the aggregation of matter—giving rise to solidity, hardness, elasticity, &c.—is said to depend, and likewise those producing gravitation and chemical attraction seem not to be conceivable as states of motion of the body affected. How then are these two kinds interchangeable, and how can they be brought under the law of conservation and subjected to the rigid condition of expenditure of themselves in exact proportion to the result produced? How, again, can statical equilibrium and potential energy be conceived of as modes of motion? One of the strongest arguments for the dynamic theory of heat was the impossibility of conceiving matter in a latent state, but are we better able to conceive latent motion? As long as the nature of "interior work" (§ 17) is inscrutable, how can we understand what the motion of heat is doing when it becomes insensible to the thermometer, as the greater part of the specific heat of water is. In short, how can we ever conceive the active forces to be in a state of rest, or the apparently fixed attractive forces ever to be in a state of motion? We can see how ordinary motion may be stored up as motion in a fly-wheel, and if we knew the true nature of all the other physical forces, the difficulties above suggested might disappear. As an illustration of this possibility, and to indicate more precisely the real nature of the difficulties which surround the definition of force, we may touch shortly on some hypothetical solutions of these obscure points; premising, however, that Mayer must not be held accountable for them, as he consistently avoids all speculative questions of the kind.

§ 51. The first difficulty is how to reconcile static equilibrium and the force of chemical combination, elasticity, and gravity, with the doctrine of conservation of force.

When two (non-elastic) equal bodies in motion with equal velocities meet exactly, the motion is exactly neutralised and both bodies are brought to rest as regards visible movement; but we are told to look further, and we find that the exact sum of both their motions is not destroyed but transferred to the molecules, thus becoming the motion we call heat. What, then, takes place when two weights exerting a pressure capable of causing the above amount of motion exactly balance each other? or when two springs are kept pressed against each other with the same force? Are they not exerting the same force if that means "what tends to produce motion?" What then becomes of the force? Where is the heat or other force into which it is transferred? we know there is none. Is there, then, really no pressure-force exerted? Undoubtedly there is, for if we cut away one weight the other starts into motion immediately with its full power. Perhaps some light may be thrown on this question by the recent theory of the elasticity of the gases.

Suppose a moveable piston placed in a horizontal tube closed at both ends, and kept in the centre by an equal volume of air on each side. Here the piston is at rest in a state of statical equilibrium, being maintained in that state by the elastic pressures of the air on both sides exactly balancing each other. But what is here the mechanism of the static pressure? According to the present theory of gases it is produced by the perpetual impact of air-molecules on the surface of the piston; it is, therefore, an infinity of small motions at a certain measurable and now known velocity, but so long as the piston does not yield, the motion of these is retained unchanged as it must be by the old law of "conservation of *vis viva.*" (See appendix.) The loss of motion in a pendulum by friction is here paralleled by the loss of heat by conduction and radiation, but unlike that, is exactly compensated by the absorption of heat from the surrounding media, according to the law of exchanges. Thus the static equilibrium of gases is the result of the balancing of actual velocities. Now, if we understood the physical constitution of solids, might we not be able to account for all cases of statical equilibrium in a similar way, instead of having to content ourselves with a merely mathematical explanation by means of the principle of virtual velocities? If, now, we suppose the amount of force on the right side of the piston to be diminished either by removing some of the vibrating particles, *i. e.*, pumping out some of the air, or by abstracting some of the actual *vis viva* of the molecules, which is in fact heat, *i. e.*, cooling that side, instantly the velocities of the particles on the left side over-balance those on the right, and the piston is pushed to the right; thus the statical pressure on the left side is shown to have been potential energy. Work is now done, display of force is made, and something is expended; for the oscillating motions of the molecules, hitherto manifested as heat, are consumed in the work of moving the piston visibly onwards, and being expended, are of course lost as heat, so that the left side of the tube is chilled exactly in the ratio of the work done in moving the piston.

Another illustration of a similar character may be derived from the following passage from Bertrand, showing the distinction between Newton's force (§ 41, I) or dead, or statical, or bursting pressure, and Mayer's force, or Rankine and Young's energy.

"Let us consider a kilogram of air at the temperature of 20° C. and under the atmospheric pressure of 0·76 m.; it will occupy 0·7755 m. Its energy surpasses 25,000 kilogrammetres; that is to say, that, if without furnishing it fresh heat, you employ it to raise a piston to the complete extinction of all its elastic force, you may (admitting the constancy of the properties of the gas) obtain from it this immense quantity of work. But its total heat would then be exhausted, and its temperature would sink to the calculated absolute zero, *i. e.*, 273° below zero Cent.

"Let us suppose this same kilogram of air reduced to a volume ten times less, it is heated at first; but let it cool and we shall have a kilogram of air under a pressure of ten atmospheres, and at the temperature of 20°, capable of an *effort* (a dead or bursting pressure) ten times greater than at first; nevertheless the total *work* that you can get from it, its energy in a word, remains exactly the same; the work expended in the compression having been converted entirely into heat exists no longer in the gas now cooled; the portion of work which is necessary for forcing the molecules of solids and liquids nearer to each other during the compression, and which is given out again spontaneously on their expansion, does not exist in respect to the gases, of which the molecules are too far separate, and are in consequence destitute of mutual attractions and repulsions."—Bertrand, April, 1870, p. 232.

Here the total energy of a gas is allowed to be proportioned to its *absolute* temperature, reckoned from 273° below zero C. And for illustration it is supposed that the total energy can be converted into work, in which case it can never be more than 273+20° no matter into how small a bulk the gas be compressed, nor how great the amount of work spent on compressing it, provided this last be allowed to escape in the form of heat, *i. e.* the apparatus be allowed to cool. If, however, this last be not done and the compressed gas retain all the heat of compression, not only will its bursting power, but also its power of doing work be increased by the amount spent in compression.

In the above illustration drawn from a piston in a closed tube we have statical pressure, potential energy, actual energy, and visible movement, all represented, and all shown to depend on the *vis viva* of actual motion.

Now suppose, as Newton hinted, gravitation to be produced by ethereal pulses acting on the particles of matter pushing them towards each other, like those of the air on each side of the piston, then we should have the same state of things as regards gravity as with gaseous elasticity. Again, if we suppose the elasticity of solids to be caused by similar ethereal pulses or by orbital movements of the molecules a similar explanation would apply. But we are, as yet, far from any true theory on these questions, and in default, we must look on the storing up of force in the form of potential energy, as unexplained, and be content to accept, as a fact, that the force thereby represented is equivalent to the *vis viva* of motion expended on it.

§ 52. We have here a glimpse of the possibility that the forces may be nothing but motions of matter, if under the term matter the æther is also comprehended; and that their ultimate complete correlation may one day be established, when we shall see that they form a closed circle without beginning or end. That this is not yet apparent is owing to the break caused by our ignorance of the true physical nature of gravitation, chemical attraction, elasticity, &c. Hence arises the difficulty of a definition of force, for that must be consistent with the equivalence and possible correlation of all the forces without implying a knowledge of their true nature. In this sense Mayer's must have the preference, as it pronounces no opinion on the nature of any force, or on its origin, and is not liable to be confounded with any property of matter. The introduction of the term *energy* is no real gain in precision, though it is a convenient term when used to express "the power of doing work."

The Germans have done very well without it, and though it meets with the approval of Helmholtz and Clausius, yet it is not essential, for there is no connection in which the word force might not be used instead of it. Balfour Stewart says that a stone when thrown up to a height, and caught there, has energy of position, but when fallen to the ground has no energy but only force, meaning the pressure force of weight. It is plain, however, that, but for the intervening obstacle, it would still have energy enough to fall (say 4000 miles) to the centre of the earth, where, indeed, it would have no energy in his sense, but then also no force in the sense of weight. Thus, there is no real distinction between the two cases, and the stone is simply caught a few feet higher in one than in the other. At the centre of the earth, of course, though a body has no weight, it is still subject to the force of gravitation, which, indeed, is now at its maximum, but is equally balanced on all sides.

The potential energy of a raised weight may therefore be just as well called unbalanced falling force. Energy is therefore not something new caused by force, but is simply a convenient name for a *disposable balance* of force. The same applies to heat, chemical attraction, and other forms of force. In attempting to distinguish between potential energy and force, some authors have mixed up the question with the metaphysical one of the origin of force, and have confounded the ideas of force and property.

§ 53. Bence Jones says, at p. 35, " To avoid the indistinctness arising from using the word force at one time as the cause of an effect and at another as the effect itself, the term conservation of energy is now adopted instead of conservation of force. Thus, force, which we assume to be indestructible and inseparable from matter, is the cause of energy, and energy is the effect of force, illustrated by this diagram :

" Cause.		Effects.	
Force or power, total quantity of which is as constant as the total quantity of matter.	=	Actual energy=motion Potential energy=tension	=work."

What do we gain by this? In the very next sentence he says, " The idea of the conservation of energy is that the sum of the actual and potential energy in the world is constant." Now, if he admits this, what else can his force be but some form of the already existing store of energy, and how then can it be called the cause of energy? Or he must assert that matter by its very nature possesses the power of originating a certain definite quantity of energy, and no more, and to this power he gives the name of force. He arrives at this by a curious process of reasoning from the irrefragable position, that force is inseparable from matter, in the sense that force cannot exist without matter. This position presents no difficulty to those who look upon force as motion, for of course it is impossible to conceive motion unless there is something to be moved. But he proceeds from "the inseparability of force from matter," "the absolute union of our ideas of matter and force," and the like expressions, till he arrives by easy stages at the conclusion that it is impossible "to think of matter as inert or void of all force." He says, "As soon as it is admitted that *force is absolutely inseparable from matter*, whether gas, liquid, or solid, it will become as impossible to think that matter can consist only of centres of force as to think that matter can be inert or void of all force. *Matter, if it be inseparable from force,* must always be in a state of motion, or of tendency to or of resistance to motion, and it can never be in a state of perfect rest."

On the contrary, the majority of physicists have no difficulty in conceiving matter in a state of rest, inert and void of all force. In fact, this is implied in the idea of *inertia,* which is universally allowed to be one of the cardinal attributes of all matter. It is undoubtedly true that, as the universe is now constituted, there is no example of a single particle of matter void of force; but this is a very different thing from the above statement, and lends no countenance to the idea which underlies it, that matter must have contained an inherent store of force, which is "the cause of the energy" of the universe. We shall never escape the confusion thus engendered till we acknowledge plainly that no property of matter can originate force, or, in other words cause motion without expending force previously imparted to it. Matter must be regarded as possessing certain inherent properties, but, above all, that of absolute inertia, which enables it to take on a variety of motions when force is *furnished from without;* but it is totally unable *per se* to display any of its qualities or originate the smallest motion.* In a practical point of view, the important thing is how to distinguish between these inherent properties on the one hand and force on the other, and to endeavour to unravel the prevalent confusion arising from a loose use of these words.

§ 54. The truth is, the discovery of the mechanical equivalent of heat has, we venture to say, given the death blow to the whole doctrine of attraction and repulsion, as inherent properties of matter. These terms are still employed by physicists, but the conviction is gradually spreading that they are only convenient expressions for facts, the real nature of which is not yet understood. Just as before the discovery of the true nature of atmospheric pressure, aspiration and suction might have been looked on as attraction, so perhaps the doctrine of attraction may be no better founded than that of the horror of a vacuum. Attraction implies the production of motion, which is impossible without the expenditure of something, and that something cannot be matter or any of its essential properties. "In no case," says Faraday, "is there a pure creation—a production of power—without a corresponding exhaustion of something to supply it." The general tendency of opinion is well expressed by Laugel, who, after explaining that Newton spoke of the attraction of gravitation merely as a formula, goes on to say "When we say that bodies attract each other, we ought simply to understand that things take place as if bodies attracted each other."

And again : "To suppose in the centre of each molecule a true power of attraction or repulsion, as is often done, is a notion so strange, that no one could comprehend how it became familiar to so many minds, did he not know how prone we are to give to all objects something in common with ourselves."

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* On the first law of motion Goodwin says—"With regard to the meaning of this law, it is intended to assert that there is in matter no tendency to motion of one kind more than another, or, indeed, of any kind ; that matter is purely inert, and the cause of any motion which a body may have is to be sought not in the properties of the body itself, but in external influences." (*Mathematics*, p. 269.)

Further, "There are in reality no entities of this kind, nor fluids, nor attractions, nor repulsions; reason merely presents to us matter animated with movements; some of these movements are perceptible to us, and make up the relative and incomplete notion which by the aid of our senses we form of materiality" (p. 217).

Such comprehensive views of the universe were to a certain extent anticipated by the profound intellects of the seventeenth century, and, in fact, Bertrand entitles his elaborate disquisition *Renaissance de la Physique Cartesienne.*

§ 55. Matter without force would be a true state of chaos. It would then have no aggregation nor any of the secondary properties which that involves. There could be no chemical reaction, nor any of the phenomena of gravitation, heat, light, electricity, or magnetism, nor could life exist. Such a state is, however, quite conceivable. Matter would then exist in full mass, though no weight would be possible; the elements would be there in full tale with their distinct chemical affinities, though these could not be manifested. In short, all the inherent properties would be there in all their completeness, and might so remain for ever without being called into exercise. This last constitutes another grand distinction between property and force. The former may be in exercise, and then may cease to be in exercise, without our requiring to ask what has become of it. Not so with regard to force; you must follow it;* once in operation, it must be accounted for in some way or other, it must be traced through its transformations into some other force or into statical equilibrium. The conception of matter emptied of all force is not more difficult than that of the absolute zero of heat, which is now a postulate in physical science. As to the origin of force, the question, like that of the origin of matter, is a barren one, and to indulge in speculations thereon is only to "darken counsel by words without knowledge."

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* "The case of a force simply removed, or suspended, without a transposed exertion in some other direction, appears to me to be absolutely impossible." (Faraday, *Phil. Mag.*, iv, 13, p. 237.)

§ 56. On the question of property and force, Dr. Bence Jones has, I am inclined to think, entangled himself in a fallacy which may be the source of those purely chemical views of biological phenomena which he holds, and which are so repugnant to the vitalist school. He leads up to the subject by dividing the course of opinion respecting the connection of matter and force into three stages, as follows:

> 1st. That in which there is a complete separation between the ideas of matter and force. As an example of this he cites the Mosaic record of the creation in which, for instance, light is represented as created by itself, independently of the earth and water, &c.—"that is, light or force was regarded as perfectly separable from matter." The second stage is marked by the incomplete separation between the ideas of ponderable matter and force. Here "force is considered as altogether separable from ponderable matter, but actually to consist of or to be perfectly inseparable from an imponderable æther, gas, or fluid, which is capable of being attached for a time to the ponderable matter." The authorship of this idea he ascribes to Kepler, and as its partisans he names Descartes, Liebnitz and Newton. He names this the Newtonian stage of ideas, chiefly owing to Newton's strong advocacy of the emission theory of light, which for long was maintained on his authority, and with it the imponderable-fluid theories of heat and electricity.
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> The 3rd stage "is characterised by the complete union or perfect inseparability between the ideas of matter and force." This he calls the modern stage, and one which rests solely on the advance of natural knowledge.

With every wish to do homage to one to whom medical chemistry is so much indebted, I cannot consider this a happy description of the progress of scientific thought. It cannot be admitted that, because Newton upheld the emission theory of light, he is to be put forward as the representative of the second stage, viz. that force consists of an immaterial gas. The modern theory, that the forces of gravitation, chemical attraction, and cohesion, consist of *undulatory or other movements* in a kind of matter—the interstellar medium—acting upon the particles of common matter, seems to us far more consonant with Newton's views than the representation of them given above. The dispute between the ætherists and non-ætherists does not turn on the question whether an æther exists or no, for even the non-ætherists are compelled to admit the existence of an interstellar medium of some kind, but as to what is its nature, or even what it shall be called.

§ 57. In illustration of his views, Dr. Bence Jones quotes part of the following passage from Faraday's paper "On the Conservation of Force" (*Phil. Mag.*, iv, 13, p. 235):

> "What is called chemical attraction affords equally instructive and suggestive considerations relative to the principle of the conservation of force. The indestructibility of individual matters is one case, and a most important one, of the conservation of chemical force. A molecule has been endowed with powers which give rise in it to various qualities, and these never change, either in their nature or amount. A particle of oxygen is ever a particle of oxygen, nothing can in the least wear it. If it enters into combination and disappears as oxygen—if it pass through a thousand combinations, animal, vegetable, mineral—if it lie hid for a thousand years and then be evolved—it is oxygen, with its first qualities, neither more nor less. It has all its original force, and only that; the amount of force which it disengaged when hiding itself has again to be employed in a reverse direction, when it is set at liberty; and if hereafter we should decompose oxygen and find it compounded of other particles, we should only increase the strength of the proof of the conservation of force, for we should have a right to say of these particles, long as they have been hidden, all that we could say of the oxygen itself."

The chief question in the paper from which this passage is taken is respecting the difficulty of correlating gravitation with the other forces. Here Faraday evidently uses the word force in its popular sense, as synonymous with power or capability, and not as contra-distinguished from property. This is to be regretted as tending to keep up the prevailing ambiguity, but it is plain that in the first part of the above extract by "force" he means property, for he speaks of qualities which cannot increase or diminish, while in the latter part the word "force" is used in its proper sense as denoting something that can be "disengaged."

We may notice here that an important principle is implied in the last sentence, viz. that the properties of compound bodies are as truly inherent and as little to be confounded with transferable forces as those of the simple bodies.

The conclusion of the extract is not given *verbatim* by Bence Jones, but is paraphrased in the following manner:

"If it were possible to take the ultimate atom of any one of the elements, we should find that the chemical force which constitutes and determines its nature would be absolutely inseparable from the matter of which the element consists" (p. 17).

Here we lose the idea of the possibly compound nature of the elements which gave force to Faraday's argument, and what is left is the mere truism, that the chemical properties of a substance are inseparable from it, force being here used in the sense of property. Now, though Faraday himself unfortunately speaks in this loose way, he shows that gravity, considered as a power of attraction without the expenditure of anything, is inconsistent with the law of conservation of force, and, therefore, the whole scope of the paper may be taken to be that gravity must be a force, and not a property, in the strict sense of the words here insisted on. He certainly gives no support to the arguments for the inseparability of matter and force advanced by Bence Jones, particularly in the following passage:

> "The union, also, between matter and gravity is just as inseparable as the union between matter and chemical force. Matter without weight is not matter at all; the weight belongs to the matter and cannot be taken from it. The gravity can no more be destroyed than the matter itself can be destroyed. However small the matter may be divided, yet each part will have a part of the force, and there can be no more of the force lost than of the matter. We cannot think that the matter can exist without the force of gravity being always acting or ready to act in each atom of it. Nor can we think that any portion of the force of gravity can be separated from the matter. If we mentally attempt to divide any amount of the force into its constituent portions, then every portion, however minute, of the force must have a corresponding portion of matter to which it is inherent, and without which the force cannot be thought to exist" (p. 18).

§ 58. It certainly does violence to all our preconceived ideas to suppose that weight is not an essential property of matter. In all our experience the two are indissolubly associated, and it is upon this constant connection that we depend for the demonstration of the indestructibility of matter. We may melt it, pound it, volatilize it, or pass it through a thousand different chemical combinations, and at the end we shall find it again, weight for weight, exactly as at the outset. Notwithstanding all this, it is only the association of ideas that makes it difficult for us to imagine matter as existing without weight. The facts adduced are undeniable, but the question is whether they depend on a property of attraction inherent in matter or on a force acting, like other forces, only conditionally. In favour of the latter view we have the all-important fact that weight is capable of being increased or diminished. A mass of matter weighing 1000 lbs. at the sea level loses 2 lbs. at four miles above it; if carried 240,000, *i.e.* to the moon, it would only weigh 5 oz.; and at a calculable distance in space the weight would become imperceptible. On the other hand, the inherent properties admit of no such variation—"a particle of oxygen is ever a particle of oxygen; nothing can in the least wear it." We cannot doubt, even without the evidence of spectrum analysis, that, whether on the earth or at the sun, where it is twenty-five times heavier, or in the remotest regions of space, where it weighs nothing at all, it will still be "oxygen with all its first qualities, neither more nor less." It is true that there are difficulties in the way of conceiving gravity as a force, which seem to countenance the idea that it belongs to matter as a property, such, for example, as the fact that it is not transferable, like heat, from one body to another. But this would, no doubt, be explicable if we fully understood the nature of gravity as a special force. This is a question which cannot be gone into fully here, but, as bearing on the distinction between property and force, I may cite the conclusion at which Faraday arrives in the paper alluded to in § 57, viz. that the cause of gravity is a force, not resident in the particles of matter, but in something *external* to them. In support of this conclusion he quotes the following passage from Newton's third letter to Bentley:

"That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a *vacuum,* without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity that I believe no man who has in philosophical matters a competent faculty of thinking, can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial I have left to the consideration of my readers."

§ 59. This sentence, so utterly opposed to all the ordinary notions entertained respecting Newton's theory of gravitation, seems to have made a deep impression on the mind of Faraday and of other distinguished physicists. Since the publication of Faraday's paper Waterston, Challis, Maxwell, Glennie, and others, have shown mathematically that all the facts of gravitation, as well as of other attractions and repulsions, are susceptible of explanation quite as satisfactorily by the supposition of external pressures on the particles of matter as by the prevalent one of attractive forces within them. This hypothesis of pressures (supposed to be caused by æthereal undulations or stresses of lines of force), unlike the ordinary notion of gravitation as an inherent power of attraction without expenditure of anything, does not conflict with the known laws of force.*

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* In the illustration of the coincidence of static and dynamic equilibrium given at § 51, the dead pressure on each side of the piston must rise and fall with the temperature, but no motion is thereby produced, because the pressures or velocities of impact of the air-molecules are exactly balanced. The parallel of this is observed with respect to gravitation. A mass of matter will weigh a known amount less at the top of Mont Blanc than at the sea level; but this must be measured by a spring balance, for with the scale-beam the loss of weight of the mass would be exactly balanced by a similar loss undergone by the counterpoise. The mathematical physicists who have worked out the hypothesis that the force of gravitation resides, not in common matter itself, but in waves of translation in the æthereal ocean, have shown how such a physical assumption is capable of explaining all the laws of gravitation, viz. such as the apparent attraction in the inverse square of the distances and in the ratio of the mass, and also how it can act through an intervening body. It enables us also to understand how something of the nature of motion may

Doubtless some corresponding property in matter is implied to admit of the display of the force of gravitation. This is simply the capacity possessed in an equal degree by all matter of being moved by the force of gravity. Here we must perceive that matter is passive throughout, and there are thus two classes of forces, viz.—1st, those which cannot be supposed to consist in the motions of the matter itself, but reside in something external to it, by which it is affected passively (*e. g.* gravitation and chemical attraction); and 2nd, those in which the movements of the matter itself constitute the force (such as heat, electricity, &c.). These last may mostly also transfer their motion to the æther or reciprocally receive it, *e. g.* heat may consist of vibratory movements of a hot body, which may be transferred from such a body to the æther, as radiant heat; which last, again, may be retransferred from the æther to common matter as when the radiant heat warms another body. No corresponding phenomena occur in the case of gravity and chemical attraction.

§ 60. Now, whereas the force of gravity acts equally upon all matter, the force of chemical attraction is limited to the action of certain kinds of matter upon each other. Here also two distinct factors are involved, viz. the property of chemical affinity on the one hand and the force of chemical attraction on the other, the product of these two being chemical combination. The act of chemical combination implies a certain amount of motion, which, again, involves the expenditure of *something*, and this something cannot be the inherent property which constitutes the chemical relationship or affinity between the combining bodies, because that property remains undiminished and unchangeable. What

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be expended in producing the motion of a body falling to the earth. But it does not give any hint as to how this force is replenished any more than heat and light, which are incessantly being dissipated at such an enormous rate. Here, however, Rankine's hypothesis of the reconcentration of the energy of the universe irresistibly recurs to the mind, and the question has been asked—If it is not tenable in the original form (see p. 8), may it not be in some modification? may it not be that when the undulations of light or heat reach the margin of the æthereal ocean they are transformed and reflected as waves of translation, thus becoming the cause of gravity?

is expended must, therefore, here as elsewhere, be *force.* The inherent property in this case cannot, any more than in that of gravity, be in itself a power of attraction, but only determines the effect of the acting force. This force is the same for all chemical combinations, and is therefore called the force of chemical attraction, just as we speak of the force of gravitation, but it is the property of affinity which determines what particular bodies shall combine with one another, using in the act of combination the requisite amount of the common stock of force. No amount of the force of chemical attraction will cause the combination of bodies destitute of affinity for each other. To speak of the force of chemical affinity already implies a hypothesis which the foregoing considerations show to be erroneous. In strictly accurate language we should say every chemical combination involves the existence of the property of chemical affinity and the expenditure of the force of chemical attraction. What the nature of this force of so-called attraction may be is not decided, but it probably is caused, like gravitation, by pressure from without upon the particles of matter. In what relation it stands to gravitation and the other forces is a subject on which it would be premature to speculate.

In a chemical combination—oxidation, for example—the force that is evolved is merely common force, transferable, transformable, and commensurable in all forms; in this case it is the force of chemical attraction assuming generally the form of heat or electricity. Also, when we dissolve or reduce a chemical compound we must furnish the exact amount of force necessary for the potential energy of the components in their elementary state; but what we furnish is only common force, such as heat or electricity, not any *special* force, which gives the elements (say oxygen and hydrogen) their special chemical qualities, *e.g.* the faculty of combining in definite proportions, and forming a third substance with totally different properties. Such a faculty is the inherent property of those two kinds of matter alone, and cannot be transferred to any other substances in the universe; it cannot, therefore, be a force at all in the strict sense, and this distinction is quite easy to conceive, although, in point of fact, no display of chemical affinity nor any actual combination can take place without, at the same time, the co-operation of "that which is expended in the production of motion." So whatever part force may play in chemical combination, there must always be an inherent property in the several elements which determines it, and force alone cannot be the sole agent, but only one factor in the process. The conclusion, therefore, is that chemical affinity is a property which can be satisfied but not consumed; while chemical attraction is a force which must be expended and changed into another form in all combinations, or furnished from without in another form to be stored up as potential energy in all decompositions.

§ 61. As we approach biology and endeavour to explain Dr. Fletcher's theory of the dependence of vitality on a state of combination of matter entirely *sui generis*, and not on the addition to ordinary chemical compounds of either a substantial entity called the vital principle, or a force or forces called vital, it will be necessary to enter still farther into the question of the distinction between the properties and forces of matter in order to see what foundation physiologists may find in pure physics for the last hypothesis. For, in fact, the theory of vital forces is little else than the old vital principle in a new form, supposed to be more consonant with the present state of physical science.

The phenomena of life are supposed to be produced by the action of the proximate principles, viz. albumen, syntonin, protagon, gelatin, &c., animated by a peculiar force or forces called vital, which, however, must be drawn from, and are commensurable with, the ordinary physical forces. In short, that, for example, albumen, gelatin, &c., by the addition of certain forces, can put on the behaviour of living matter; just as copper, though otherwise the same substance, acquires certain powers when hot which it has not when cold, or iron becomes magnetic by induction. To this theory Dr. Fletcher's view is opposed quite as much as to the more unphysical speculation of a substantial vital principle. Does the part played by the forces in the development of the secondary properties of matter through combination and aggregation lend any countenance to this theory?

Supposing that we can predicate nothing as to the essential properties of matter without force except those commonly assigned, viz., *extension*, *impenetrability*, and *figure*, still we shall have no difficulty in recognising inherent and essential differences in the properties of different kinds of matter by their reaction under the influence of the forces. It is not necessary to adopt either the Newtonian (of hard, round particles) or the Boscovichian (of points of repulsion), or any other theory of the nature of the ultimate atoms of matter; suffice it to say that they cannot be either solid, fluid, or gaseous, for these terms imply the existence of aggregation under the influence of forces. We can quite well conceive a single particle of matter which, of course, cannot be solid, or liquid, or gaseous; therefore, if these states are called properties, they must be called secondary properties, inasmuch as they can only be displayed by the intervention of, or a union with force. By some physicists they are called *acquired* properties, which admits their position; for, of course, an inherent property cannot be acquired any more than it can be lost. This development of secondary properties by union with the forces, leads up to the vast variety and complexity of the phenomena of nature. As the universe is now constituted, as far as we know there is no matter which is not under the influence of one or more forces. If we extend the second law of motion (see Appendix) to the forces in general, we perceive how the same body can be influenced by a variety of forces at the same time without interference with each other. Probably an elemental gas is the simplest form in which a mass of matter can exist in respect to force, for its particles are removed from the sphere of molecular attraction; but even it is animated with the forces of heat and gravitation, and the terrestrial and orbital motions common to all matter in our world, besides the whole store of potential energy of chemical attraction. A solid is, in addition, under the action of the force of cohesion, and other molecular forces or velocities producing the secondary properties of solidity, hardness, tenacity, elasticity, &c. Some bodies, in addition farther, may take on the peculiar molecular motions constituting electricity and magnetism. It would seem that in this enumeration we comprise the whole of the properties of any substance, and, as we do not know anything of the properties of matter except through the forces, we are tempted to consider that it is nothing but the addition of forces which gives to matter its modifications. A simple test will, however, disprove such a notion. Suppose iron the solid spoken of; the mere increase of the molecular velocities of heat will disperse its particles into vapour, and deprive it of the majority of the above secondary properties; while to other tests it will be palpable that iron is iron still, and distinct from all other elements. In like manner we can suppose iron emptied of the forces of heat and gravitation and mechanical motion in space, and it would still be iron in all its inherent properties, and it would certainly neither have been annihilated nor become anything else. Nevertheless, as we do know and can know nothing of the inherent properties of matter except through the forces by which they are displayed, the tendency is almost irresistible to put these latter into the foreground and lose sight of the inherent properties. It is true that solidity, fluidity, hardness, softness, elasticity, temperature, crystallisation, magnetism, &c., depend on certain forces acting in certain amounts on and in matter, and these states could not possibly exist without force; yet it is the properties of matter, whether simple or compound, which are the determining power or agency which decides whether the amount of those forces furnished shall produce these states in any given condition, or whether the forces shall take effect at all, or even in what forms force itself shall exist. According to the hypothesis of æthereal velocities the forces on which gravitation, cohesion, and chemical attraction depend, and to which matter is passive (§ 59), must be held to be diffused throughout the universe, and all kinds of matter are enveloped in them, but they take effect on none but those whose special qualities fit them for their display, whether at all or in what degree. Just as our solar system is full of radiant light, while to us the heavens at night present the blackest darkness except where the waves of light are reflected by a planet or satellite. To return to a special example. At a given temperature iron will be a hard, tenacious solid, with its molecules firmly bound together (whether in static or dynamic equilibrium) by the force of cohesion; whereas, at the same temperature, the particles of hydrogen, destitute of all the secondary properties of cohesion, will be flying from each other with inconceivable rapidity. Why is this except that the inherent properties of those substances are different, and it was they which determined how much of the force of cohesion each substance would take on, and thus acquire its secondary properties?

In respect to its secondary physical properties, we say of iron that it excels all bodies in tenacity, it is ductile, malleable, and capable of being welded at red heat; its specific gravity is 7·7; it has a grey colour and metallic lustre, and at temperatures below redness is attracted by the magnet. Now, none of these properties can be displayed without force, and they are all mere amounts of forces, or behaviour under forces, which can also be displayed by other bodies. One or more of these secondary properties may possibly be found in exactly the same amount in other bodies, and it is only the union of a sufficient number of these properties in the exact proportion which enables us to distinguish iron from other kinds of matter. Nay, say some, as we can have no knowledge at all of the properties of anything except through the forces by which it is animated, so what need have we to suppose different kinds of matter at all when it would be sufficient to endow one kind with the exact proportions of each force to give the secondary properties, which are our only means of recognising different kinds of matter? Others, again, go even farther and say what need of matter at all? let us have merely centres and mixtures of forces in due proportion, and thus we shall have all the variety of the material universe. To such speculations the course of physical science seems to me wholly opposed, and it is impossible to imagine the existence of definite mixtures of abstract forces, nor, indeed, abstract force at all. As above said (§ 53), force is inseparable from matter, and consists merely of motions or affections of matter; and it is, therefore, absurd to suppose that the properties of the latter have not their share—and, in fact, the chief share—in determining the mode in which the effects of the union of matter and force shall be displayed. In truth, we have very little power over the transformations of force, and, for the most part, none at all over the proportions of the forces required for the secondary properties of matter. Under the pressure of one atmosphere we may furnish heat to ice, and melt it at 32° F.; but we cannot alter its amount of cohesive force so as to cause it to melt at any other temperature. And in respect to the conversions of one form of force into another which is the theme of such constant talk, we can do little more than furnish the equivalent amount of actual energy and take advantage of the properties of different kinds of matter which transforms it whether we will or no. Illustrations of this would fill a chapter of themselves, so I will leave the subject for the present, as it will come in again incidentally in giving examples of the equivalence and convertibility of the forces. What has been said of the secondary properties of the elements applies even more strongly to their chemical compounds. And to characterise the distinctive properties of an element you have not only to describe its physical properties, but also enumerate all the combinations it can enter into with their characteristics also. Whatever be the nature of chemical attraction there must have been something peculiar in the nature of oxygen and hydrogen, alone of all bodies in nature, which enabled them under its influence to combine and form a third substance totally distinct in secondary physical properties from either. And however peculiar and wonderful the properties of water may be, it is plain the potentiality of them must have been contained in the inherent properties of oxygen and hydrogen. No one would now revive the idea of the essences or principles of 'aquosity,' or 'saccharinity,' or 'acidity,' or 'alkalinity,' nor ought we to talk of chemical *forces* in any way corresponding to these ideas. It is of importance to have our minds clear upon this subject as we get nearer to physiology, so I again would urge that the word force is misapplied when used to denote the distinctive chemical qualities of substances. The contrast between properties and forces thus remains in respect to compounds just as strong as with simple bodies, if such there be. Above all, there belongs to the properties of compounds, as well as elements, the distinguishing feature that they cannot be communicated.

To give one body the characteristic properties of another, it must, if an element, be transmuted, or if a compound, converted, into the same. By the operation of the active forces we have no power of changing the properties of matter, and we have even very little over chemical combinations, except in the way of dissolving them. On the discovery of the decomposition of water and the alkalies by the voltaic battery, great hopes were excited that new powers of synthesis as well as analysis would be put into the hands of chemists, and, in fact, several combinations were found to take place under the influence of the electric spark. But these hopes have proved fallacious, and it is now admitted that, although the action of heat and the electric spark and the various agencies included under the name of catalysis are not fully understood, the forces in these cases have no direct power of causing combinations, nor does any special force at all exist in catalysis. The probability is, that in some way the particles of the elements are simply brought within the distance of molecular attraction, as happens in solution, and the combination takes place solely in virtue of their chemical affinity. Thus the active forces, and whatever it may be that is comprehended under the name of catalysis, can at the most set a going chemical action in bodies possessing already the full amount of affinity and potential energy to complete the action. This applies to the great majority of instances, but in the synthesis of chlorine and hydrogen by light, phosgene gas, and similar compounds, a quantity of the chemical rays is extinguished proportional to the chemical effect produced. So probably in the inorganic world we have an example of potential energy of the new compound being furnished by light, as we see on such a large scale in the vegetable kingdom. Still it is the chemical properties of the elements which make use of light rather than that light compels them to enter into a union of which they are otherwise incapable. The conversion of *vis viva* into potential energy of the compound in the synthesis of inorganic bodies, will be farther noticed under the question of molecular velocities. The almost infinite variety of the material universe depends on the secondary properties of matter developed by chemical combination determined by the inherent properties of the elements. No knowledge of the powers and properties of matter we now possess enables us to place a limit beyond which new powers may not be developed by new combinations and conditions. Therefore, if we are to discover the sources of any new and peculiar powers, such as vitality, we must search for them among the properties of matter, and not in new forces. The active forces are comparatively few, and, though indestructible, are convertible and transferable from one body to another, and in the form of heat capable of dissipation as far as our world is concerned. On the other hand, the properties are peculiar, inherent, incapable of dissipation, and incommunicable. The convertibility of the forces must be understood in a sense to a certain degree restricted, for our power of converting motion or any active force into another is dependent on the properties of the matter affected. Thus the stoppage of motion by friction in homogeneous bodies produces heat, and between heterogeneous bodies, electricity. Magnetism is not directly convertible into heat, but when by its attraction motion is generated, then, by the stoppage of that, an equivalent amount of heat is generated. Iron in a state of cohesion under the motion of intense heat will take on the vibrations giving rise to light, and thus convert heat into white or compound light, but if still further heated and converted into vapour, will give out a ray of one characteristic colour, and the same is observed with other elements. Some bodies, called opaque, are said to absorb light and convert it into heat, that is to say, their molecules take on the undulatory motion of the waves of light whose motion is thus stopped; they cannot, however, give it out again as light, but in the different kind of vibrations we call heat. Again, certain bodies, such as sulphate of quinine, called fluorescent, absorb the non-luminous highly refrangible rays of the spectrum, and give them out in a lower degree of refrangibility, but still light. In these cases, the properties of opacity and fluorescence, for example, resided already in the opaque or fluorescent body, and were merely called into action by the mode of motion acting upon it. We are, therefore, surprised when we see in Carpenter's early paper on the application of Mayer's Doctrine to Physiology, light spoken of as the *organizing force* in the growth of plants. In the case of sulphate of quinine, light is certainly not a fluorizing force in the sense of making it a fluorescent body, for that property pre-existed. And in the same way, though light furnishes the force essential in the growth of plants, yet the power of so using it pre-existed already in the living matter of the plant. The author, before the end of the paper, changes his view, and acknowledges that besides external forces acting on living bodies there must exist a *directive agency*. This is of course the whole point in the question of life, and as far as we have gone, analogy from the inorganic world does not encourage us to look for that in any *force*. The sense in which the convertibility of the forces is to be understood is further shown by the attitude of the fixed attractive and repulsive forces, the causes of which being unknown, we cannot say that any such conversion takes place. In these (see § 41, II, and § 45), energy or working power consists in a dead pull or pressure *and distance to act through*, and when the latter is exhausted the energy is gone, though the pull or pressure remains as strong as before. The conversion of an active force, such as motion or heat, into one of these forces, such as chemical attraction or gravitation, does not take place directly (and if at all we know not how), but the active force is expended in counteracting the attractive force to such a degree, that sufficient space to act through is given to reproduce the exact amount of force expended. When the pull of gravitation or chemical attraction is allowed to act through a working distance, it is the motion thus communicated to the masses or atoms rushing together (see Joule's first paper, § 29), which when stopped becomes the vibration of heat; and this is, as we now know, equivalent to the energy expended in raising the weight, or decomposing the chemical compound. Doubtless, as before said, when the physical nature of the attractive forces is known, we shall see that something is expended when they produce motion, and also how they are correlated with and replenished from the other forms of force (see note to § 59). We thus perceive that throughout nature, though the amount of force or energy remains constant, the particular form in which it shall display its activity or be stored up as potential energy is determined by the properties of matter. At § 53 I have given reasons for dissenting from Bence Jones's view of force as the cause of energy, and will now, therefore, propose a modification of his diagram, premising that the origin of force must be regarded as equally inscrutable to the human intellect with that of matter, both being referred to creative acts of the Almighty.

$$\begin{array}{cc} \text{The properties of matter,} & \text{Force, actual and potential,} \\ \text{according to its kind} & \text{in all its forms, probably} \\ \| & \| \\ \multicolumn{2}{c}{\underbrace{\text{Determining powers} \qquad\qquad \text{Modes of motion}}_{\text{Action or work.}}} \end{array}$$

§ 62. As a familiar illustration of the relations of the properties and forces of matter, we may compare the former to the bricks, and the latter to the mortar in the building of a house. Without the mortar the bricks cannot be made to take or retain the required form. But when the mortar is once used its faculty is lost, and it cannot be used again without a fresh store of moisture and other qualities being given to it. So likewise with the forces, for in all work done some part of them is transformed into heat and dissipated. But the properties are like the bricks, which remain there in their pristine completeness, and may be taken down and put together as often as we will, without losing the faculty of "edificiability." So likewise with all the secondary properties of matter developed by combination. The potentiality of them must have lain in the elements before the compound was made, and will continue in them when it is dissolved. There are many chemical compounds made every day which never existed, and by taking advantage of the play of chemical affinities, there are still many more to be discovered which do not now exist; yet the potentiality of all these must have existed in matter since its creation, unimpaired, although never called into exercise. When such a compound is decomposed, we do not need to inquire what has become of its peculiar powers any more than we need to inquire what has become of the "edificiability" of the bricks when a house is taken down. Nor, if by some peculiar combination of matter it should possess *vitality* as a property, do we need to inquire what has become of that when the combination is dissolved, or what we call death takes place. It is otherwise with every form of force, and if life be in any way supposed to be due to the action of a peculiar force, we are bound to show an exactly equivalent outcome of that force after death among the actual or potential energies of the inorganic world. This argument is used with propriety by the vitalist school against those physicists and chemists who speak of life as a force or forces, although it has no weight against those who look on vitality as a property of matter in a peculiar state of combination.

CHAPTER III.

ON THE NATURE OF LIFE.

§ 63. We are now prepared to comprehend the position of Dr. Fletcher in the great question of the connection between life and organization, which I may notice in this place, though I hope to be able to set it forth more in full on some other occasion. His views are expounded in a series of chapters which, for learning, wit, close reasoning, and splendour of diction, have been seldom equalled, and, I venture to say, never surpassed in the literature of physiology. He first gives the grounds for dissenting from the opinions of the large party who hold that the proximate principles are associated by common chemical affinity, and are still liable to be affected by its laws, although considerably modified by the powers of the living body, however these may be named or conceived of. He agrees with that of Berzelius, one of the few chemists who hold such an opinion, and a large party of physiologists, that in the living body the elements obey laws not merely modified, but entirely different from those of ordinary chemical affinity. Among those who hold this view are comprised both upholders of the hypothesis of a substantial vital principle like Dr. Barclay, and those who deny its existence like Dr. Prichard. Dr. Fletcher is one of the most powerful opponents of the vital principle as a substantial entity; or of any thing, of whatever nature, added to matter above and beyond the inherent properties and forces with which inorganic matter is endowed. The true nature of force was not then understood, but it could easily be shown that his arguments are equally valid against the supposition of a special force added to organized matter which could reverse or overrule its chemical laws. On the one hand, he is thus totally opposed to all theories professing to account for the phenomena of life by the action of ordinary chemical affinity and of the physical forces; while, on the other, he is equally opposed to the addition of any substance, material or immaterial, or any force not correlative with the known forces. How, then, does he account for the marvellous difference between the powers of the body living and what is apparently the same body dead? Simply by pushing deductive reasoning to its legitimate conclusion. We have no knowledge of the nature of anything except by its properties, and no knowledge of these but by their effects. When, therefore, we find living matter performing functions which the chemically combined proximate principles found in it after death are totally unable to perform; and when we find that the reagents which act on those proximate principles have no effect at all, or entirely different effects on the living matter, then the inference is simply that none of those so-called proximate principles exist in the living state of matter at all. That, in short, no albumen, fibrin, gelatin, syntonin, protagon, &c., exist in the living matter, while in their place there is a peculiar combination isomeric with the sum of these, and which is resolved into them in the act of death. The result of this organism, or peculiar combination, is a property to which we give the name of vitality, and which, when acted on by certain stimuli (the ordinary active forces among the rest), and furnished with pabulum (ordinary, chemically-combined, dead matter), performs the functions we know as vital, the most characteristic of which is that of converting heterogeneous matter into a combination generically like to itself. It is thus a self-producing combination, whether in the form of renewal of tissue, or of germs which reproduce an individual. We are thus immediately thrown back to the question of origin, viz. whence was derived the first specimen of organized matter if all we now know is derived from pre-existing similar matter? This is not quite the same as the origin of matter and force, because for those who require no substantial vital principle, and no peculiar force not to be correlated with the known physical forces, it is not a question of *creation*, but merely of *arrangement* into a new and peculiar form we call organism, *i. e.* the process of organization for the first time. Although Fletcher belongs to this party, he does not hesitate to recognise the special interference of the Almighty in the primitive act of the organization of living matter; for the spontaneous interaction of the ordinary chemical and physical forces is totally incapable of such a process. To the alleged facts in favour of heterogenesis [abiogenesis] he replies that no doubt some unsuspected source of fallacy had been present; such, for example, as that either germs are capable of resisting heat or destructive agents to as great a degree nearly as the organic matters themselves, or they had obtained entrance after these destructive processes were applied. These, I apprehend, are very much the opinions of the majority at the present day, in spite of the experiments of Pouchet, the Italians, and even those of Dr. C. Bastian made known within the last few months. Perhaps I may be permitted to add here for myself that if it should finally be established by experiment that in the passage downwards to simpler compounds certain colloids of high potential energy and complex composition should partially fall into organized states of low form by chance, or even if they must of necessity do so, it would not invalidate Fletcher's view of the nature of life. We know that there are many chemical combinations which require such designed manipulation of the play of chemical affinities that we should regard their existence absolutely impossible when the elements were heterogeneously mixed, and allowed to follow their strongest affinities. In fact, there are many such compounds made by the chemist, and which do not exist in nature. But, also, there are some such which are exceptionally found as natural products; and moreover there are some, such as the diamond, which cannot yet be imitated by the hand of man. That a living organism, however, should be so produced is, I cannot but think, even after all the experiments of the most recent times, to the last degree improbable, and I feel confident that some source of fallacy in the above experiments will, ere long, be discovered. If not, we must bow to the facts and take up another position which is not incompatible with the main doctrine here brought forward.

§ 64. Fletcher teaches, as above said, that in the living state none of the proximate principles exist as such, but only the same elements as these compounds contain, brought and held together by a power quite distinct from common chemical affinity; and that it is only at the instant of the cessation of the vitality of each organized tissue that "these compounds or reputed proximate principles are formed—at that instant when the power called chemical affinity succeeds another power, which may be called vital affinity, and by which it had previously been superseded, and common chemical compounds are all that is left of that organized mass into which the elements had been associated." "Organized matter, as, on the one hand, possessed of properties which have no parallel in such as is unorganized, and on the other, destitute of those by which the latter is characterised, must be regarded as quite distinct from it: and chemical analysis accordingly must be considered as useful in showing us, not what such matter *was* composed of while it possessed vitality, but what it *is* composed of afterwards" (p. 135). On these grounds all applications of chemistry to the explanation of physiological and pathological changes are shown to be completely untenable or of most limited scope, as merely supplying material which the organism has more or less facility in transferring to its uses. Let us imagine matter under an influence as powerful as the voltaic arc in decomposing all compounds and reducing them to their elements; and at the same time one infinitely transcending human skill in the production of new syntheses, and we shall have then but a faint conception of the extreme discordance that exists between the state of living matter and dead. In the former we must suppose the chemical compounds forming pabulum reduced to their elements, which are then rearranged into new forms by the vital processes of nutrition and secretion. Fletcher scouts the notion of even those kinds of pabulum whose proximate principles are nearest to its own, being merely absorbed and deposited again little changed: on the contrary, so complete is the decomposition and recomposition caused by the action of the living matter, that he thinks facts are not wanting which tend to show that even the inorganic so-called elements may possibly be decomposed and recombined in a transmuted state in the products of secretion. With such ideas, as may be supposed, he is diametrically opposed to all chemical physiology and pathology, which indeed are with him frequently the theme of animadversion and often of ridicule.

Viewed in the light of the modern doctrine of the nature of force, Fletcher's theory of living matter must on the whole be looked on as in harmony with it, and his modes of expression in general remarkably correct. He almost always speaks of vitality as a property of organized matter, which latter is held together by an affinity called vital. In life, he says, "the organized tissues are in their chemical natures entirely *sui generis*, and held together by vital affinities—not composed of sodo-albumen, gelatine and so forth, and held together by common chemical attraction" (p. 144). Again, irritability (vitality) is a property of organized or living matter; as characteristic of this as inflammability is of phosphorus, or elasticity is of ivory." To these expressions little exception can be taken; and I venture to say, that if we keep in mind the distinction between property and force insisted on at § 60, the breadth between vital and chemical action need not be considered so wide after all. For we may admit that the *force* of chemical attraction which holds together all kinds of compounds both vital and common chemical is the same, while the property of affinity may differ to any conceivable degree. It is nothing unusual in inorganic chemistry to find the properties—including a wide range of chemical affinities—of a compound differing entirely from those of its elements. On the contrary it is rather the rule, and the more complex the compound, the more are their differences developed, so that we need not find it inconceivable that the very complex combinations of the organic world should be possessed of such a very peculiar property as vitality. It is true we have no analogy in nature of such an extreme leap in development of properties. Granted: the instance is *sui generis.* What more can we say; the world contains only two kinds of matter, the living and the dead, so where is the room for any close analogy? Now, if we admit with Fletcher that no chemical phenomena in the ordinary sense take place in the living matter, *i. e.* there are no oxidations, nor formations of acids or alkalies, nor salts, nor of albuminoid, fibrinous, fatty, gelatinous or other matters, and that these take place only in the act of passing into the dead state, how does that affect the question of force? Not at all, I apprehend, for if albuminoid and fatty matters and oxygen collectively possessing a high amount of potential energy go into the living state and come out again as carbonic acid, water and urea collectively, with so much less potential energy, the difference will be given out as force in some form exactly the same, whatever be the intermediate states of combination through which the materials pass. It has been experimentally shown by Joule and others, that exactly the same amount of heat is extricated in the combustion of carbon, whether it is burnt at once to carbonic acid, or passes through the stage of carbonic oxide. And it is a general law of the extrication of force by chemical combination, that when a combination can take place in several different ways, the total heat evolved is independent of the mode adopted. But there are some apparent exceptions which must be noticed, lest we should be led into the fallacy of the extreme vital-principle school, that the living body is absolved from the ordinary laws of matter and force. It was found by Dulong, Favre, and Silbermann, that when carbon is burned in protoxide of nitrogen, it gives out *more* heat than when burned in pure oxygen. Again, a similar anomaly is seen in the case of formic acid, according to M. Berthelot. This substance may be considered as a compound of carbonic oxide and water, and when burned is resolved into carbonic acid and water. Nevertheless, during combustion it gives out more heat than can be produced by the combustion of the amount of carbonic oxide it contains. A more remarkable example even is given by the carburet of sulphur, which may be formed by the direct combination of the elements. This substance also in burning gives out more heat than would result from the combination of its elements separately. In all these cases heat is given out by the decomposition of the compound, and this is added to that of the combustion of the simple substances. But it also follows that force or *vis viva* must have been consumed or rendered latent in the formation of those compounds. In what form the force exists is not exactly known, as we do not know the nature of "interior work" in most substances: probably it exists as intra-molecular atomic velocities. But however this may be, the application to our subject is obvious, for in the extreme complication of organic compounds, some of which are of higher potential energy than the pabulum from which they were derived, it may quite well happen that a storing up of force may take place. Now in the calculations of Mayer, alluded to in § 23, no allowance is made for any such surplus, but the simple heat of combustion of the carbon is made the basis of calculation. So if at any time a more minute investigation should show for a time more force exerted by the living body than the oxidation of the elements accounted for, that must not be put down to *creation* of force by the vital powers. In Dr. Frankland's* careful experiments, this source of fallacy was avoided, as far perhaps as could be, by burning with chlorate of potash the substance of the muscle itself, and allowing for the evolution of heat caused by the decomposition of the chlorate of potash which acts in this way like the above-cited substances.

§ 65. If we can thus conceive of the state of organism as one which does not conflict with the known properties and forces of matter, we have simply to study it as a thing quite apart from the ordinary chemical reactions of matter in order to learn its true nature. The science of physiology is therefore totally distinct from that of chemistry, although both depend upon nothing more than properties and forces of matter.* The practical application immediately following is that nothing is to be judged of in relation to living matter according to its chemical nature, but solely as pabulum or stimulus, unless inert or a diluent like water. To the living matter there are no acids nor alkalies, no solvents nor constringents such as tan, no fats nor soaps, no ferments nor catalytic agents, no sugars or alcohols, no albumen, nor gelatin, nor fibrin, &c. None of all these things to act in the least like the way they act on dead matter. Only pabulum and stimuli. As pabulum, for the living matter immediately takes them all to pieces and recomposes them in its own way; with, it is true, certain limitations beyond which it cannot act at all. As stimuli, for the power of such agents in calling into action the property of vitality bears no relation to their chemical properties, but is *sui generis* and related to vital properties alone. According to Fletcher, irritability or vitality as a property of organized matter, though generically one, is subject to an immense variety of specific modifications, so that "every organized solid is not only the seat of irritability in general, but has a kind of irritability peculiar to itself." Furthermore, vitality as a property of organized matter is not yet life, but only one factor of it, requiring, like all properties, another factor to call it into action. This last is supplied by the agents called stimuli; therefore, following John Brown, he considers life an action produced by the operation of stimuli upon a susceptibility, but instead of Brown's term excitability, he prefers Haller's term of irritability. This he uses as synonymous with vitality as the property of organized matter, which last he also speaks of as *living matter*, or more frequently *irritable matter*. The presence of the stimuli is essential, and their action forms an integral part of the vital process, though they are frequently overlooked from being present simultaneously in the pabulum and in the other conditions essential to life. Of the nature of the action of the stimuli, he can give no account but that it is *sui generis*, depending on the properties of the substances exercising that power in relation to the faculty of irritable matter of being acted on by them. Now, in regard to the nature of the stimuli, most important changes in our views must be made, since the discovery of the true nature of force. At first sight we might be inclined to think that, as in the inorganic world, the properties of matter were called into action by the forces, something of the same kind happens here, and that the stimuli are simply the active forces.

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* Lecture on the 'Source of Muscular Power.'

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* Dr. Fletcher censures in strong and convincing language the errors of those who have confounded mere life with the immortal soul, and attempted to draw deductions as to the nature of the former from the substantial, though immaterial, nature of the latter. Whatever we may fondly hope or wish we cannot really *know* anything of the future life, except through revelation; from this we learn that the immortal soul is a divine gift to man alone, so arguments based upon such knowledge must be quite inapplicable to an attribute common to man and the "beasts that perish."

§ 66. But the question of the stimulus is far from being so simple. On the contrary, it involves the previous discrimination of the action of external agents upon dead from that upon living matter.

Great as was the genius of John Brown in emancipating his mind in 1780 from the all-prevalent notions of life as a substantial entity, and raising the stimuli to the position of an essential part in what he showed to be an action or process; yet he did not and could not penetrate the whole truth as to their operation. And when the system of medicine into which his theory had been prematurely elevated was exploded, the fundamental truths contained in his doctrines fell into discredit, and they have remained forgotten or undervalued, especially by physiologists. Fletcher, however, was fully penetrated by them, and never fails to keep them in the foreground, in both his *Physiology* and *Pathology*. To practical physicians, both in respect to hygiene and therapeutics, the stimuli are of the greatest importance as the chief agencies we have to any extent under our control for health or disease. Nevertheless, how little is known of the nature of their action upon living matter, as distinguished from the operation of the same substances on dead matter of apparently the same chemical composition! Although we are so familiar with the facts that we scarcely think any explanation is required, who can explain the action of a grain of sand upon the eye, or any mechanical irritant whose weight is insignificant and whose chemical composition is absolutely unaltered, while it may excite frightful inflammation and ultimately death of the part or even of the whole body? What force has been communicated equivalent to all that effect? Or when a mere drop of serpent poison, hardly distinguishable by chemical tests from mucilage, in a few minutes or hours alters the whole blood, and prostrates the vital powers even to death; to what change of the living matter the least like any known chemical change can that be attributed? In the time of John Brown it might have been a great step to refer these phenomena to the exhaustion of excitability from excess of the other factor of life, viz., stimulus. But the demands of science are greater now, and since we accept the principle that the phenomena of life are the product simply of the properties already potentially existing in unorganized matter and the forces, we are bound to show an analogy, at least, between vital and ordinary chemical action, in respect to the stimuli as well as other agents. Except as regards the nature of force, the fundamental principles of chemistry were much the same as now when Fletcher wrote: in what sense, then, could he still define life to be an action depending on two factors, viz., irritability on the one hand, and stimuli on the other? In the first place although the problem is generally so stated for practical reasons, the matter is much more complicated, and the more full definition of irritability is given as follows: "Vitality or irritability is the property which characterises organized beings of being acted on by certain powers otherwise than strictly mechanically or strictly chemically."* Thus, we have irritability on the one hand, and all other agents which act upon it on the other, which may be comprised under pabulum, conditions, and stimuli; and, as many agents can act in two or all three of these capacities, the problem of analysing their influence becomes very complicated. The category of *conditions* is brought in because there are agencies, such as moisture and temperature, which may be neither stimuli nor pabulum, but, on the other hand, they may stand in the same relation to vital as to chemical or mechanical action. In that case the only specific relations, as before said, in which external agents stand to living matter will be as stimulus and pabulum.

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* As precision is of essential importance, I give here Fletcher's glossary of the chief terms used by him:—

"*Organ.*—A part of a plant or animal more or less distinct from the rest, and destined to perform, either alone or in conjunction with others, some specific function.

"*Organisation.*—The *process* by which a being possessed of organs is formed (*e.g.* the formation of a germ).

"*Organized.*—Possessed of such structure.

"*Organogenesy.*—The process by which the several organs become perfectly distinct from each other (*e.g.* the development of a germ).

Likewise, as vitality is merely a property developed by the peculiar combination of the elements of the irritable matter, and is not dependent on the addition of any new principle, essence, force or power added to matter, its nature and laws must be in a wide sense chemical or physical, and Fletcher must be counted as one of the upholders of the physical theory of life as opposed to the vital principle school. Yet from his conception of the utterly peculiar nature of the living matter, as already explained, he must be looked on as one of the most extreme and strict of vitalists, and allowed to use the word vital as implying something totally distinct from chemical. We may, if we please, adopt the word metabolic,* or that of vito-chemical, to express this

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"*Organic.*—Appertaining to organized beings.

"*Vitality* or *Irritability.*—The property which characterises organized beings of being acted on by certain powers otherwise than either strictly mechanically or strictly chemically.

"*Life.*—The sum of the actions of organized beings, resulting directly from their vitality so acted on." (p. 2.)

* This word was invented by Schwann, and as it is adopted by Sharpey, and thus become a current physiological term in all English-speaking countries, I may be allowed to use it without affectation of novelty in words. But some words of explanation are needed, as I shall use it to express Fletcher's idea of the constitution of matter in the living state. In introducing this word, Schwann says, respecting the formation of cells, "Those phenomena may be arranged into two natural groups: first, those which relate to the combination of the molecules to form a cell, and which may be denominated the *plastic* phenomena of the cells; secondly, those which result from chemical changes either in the component parts of the cell itself, or in the surrounding cytoblas-

relation to ordinary chemical action, but, at any rate, we must claim the right to speak of vital, on the one hand, and chemical on the other, as practically distinct without continual explanation.

§ 67. A hypothesis such as Fletcher's is, in fact, essential if we expect to stand against the arguments of the vital-principlists, because sooner or later on the ordinary physical theory of life you come to a point where all similarity to chemical action fails. And that point is very soon reached, for it is the distinguishing mark of chemical combination, that in all cases each of the agents disappears,

tema, and which may be called *metabolic* phenomena (τὸ μεταβολικὸν), implying that which is liable to occasion or suffer change." (Schwann and Schleiden, p. 193).

These two powers are inseparably connected, for the plastic or formative power, which causes formation and growth of peculiar matters, "presupposes another faculty of the cells. The cytoblastema in which the cells are formed contains the elements of the materials of which the cell is composed, but in other combinations; it is not a mere solution of cell material, but it contains only certain organic substances in solution. The cells, therefore, not only attract materials from out of the cytoblastema, but they must have the faculty of producing chemical changes in its constituent particles. Besides which, all the parts of the cell itself may be chemically altered during the process of its vegetation. The unknown cause of all these phenomena which we comprise under the term metabolic phenomena of the cells, we will denominate the *metabolic power*" (p. 197).

The word plastic was originally Aristotle's, and was applied by him to that power which determines the association of the component parts of organs during the development of each, according to the harmonious laws we see in operation. With him it therefore meant originally the germinative or germinal power. The same power, after receiving many names, has been extended to the process whereby an old part is repaired, or a new part formed in after life. Fletcher calls "plastic" that power which, as well as giving form to renewal of its own tissues, elaborates the germ into the complete individual (II, 23), while at the same time he says these are all merely modifications of vital action. So, under the word plastic are included the varieties plastic and germinative. We have thus three principal varieties of faculty in vital action, which, though all inseparable from life as a whole, yet may be conveniently distinguished. I shall here use the word metabolic to express the changes of matter which occur in the living state, and also speak of metabolic affinity, metabolic change, metabolic action, the metabolic state, &c., as contrasted with the analogous but totally distinct adjective chemical as applied to those substantives. The words plastic and germinative or germinal stand for themselves alone as there is nothing analogous to them in nature.

and a product with new properties is formed. In the combination of a metal both the metal and the oxygen disappear, and the oxide is found in their place. Moreover, they disappear in an exact proportion; an acid is neutralised by an alkali; both disappear in exact proportion, and a new substance—the salt—is formed. On the other hand, when living matter is placed in contact with a chemical compound fit for pabulum, the latter is certainly decomposed, and a variety of products make their appearance, but the former—the agent of all these changes—is not consumed in the process; on the contrary, unlike catalytic agents, it even grows, and that not by accretion from the deposition of similar matters from solution around it like a crystal, but from within outwards by the decomposition of nitrogeneous matters and the selection of the elements it requires; more than that, it reproduces similar forms, and even develops itself into different forms, which have different powers of chemical change of the pabulum; in short, it displays the marvellous powers of a living being which it would take volumes to describe.

§ 68. No attempt to explain this complete antithesis between chemical and vital action by the influence of any known physical force, or any imaginable force correlative with the known forces, upon the albumenoids or other proximate principles, has had even the shadow of plausibility. Nor do the truly philosophical among physiologists, who hold what is called the physical theory of life, rest their conviction on the sufficiency of any of these attempts, but they look forward to the time when future discoveries will clear up the darkness in which the subject is shrouded, and in the meantime they reject the teleological view of a principle, essence, or power added to the proximate principles when organized and withdrawn at death which can enable them to perform all the wonderful functions of life, as inconsistent with sound philosophy, and calculated to stop all progress in science. The hypothesis of Fletcher—viz. that this living matter is a compound wholly different from, although isomeric with, the *sum* of the proximate principles contained in it after death, and in virtue of that peculiar mode of combination possessing the property of vitality—is, doubtless, impossible to submit to the test of direct experiment, but it must rest on deductive reasoning and collateral evidence. For it is not that no test has been as yet found which will react with albumen as with living matter, and *vice versâ*, but no such test can possibly be found.* All tests which act chemically must react by double decomposition and mutual combination with the living matter, an action which we have just said never takes place with the living matter till it has been destroyed by excess of its own peculiar action. Chemical tests can, therefore, teach us no more than destructive distillation or any mode of destructive analysis. The true tests of the nature of the living matter are the reactions it displays with external agents in the form of pabulum and stimuli, and these reactions are nothing else than *life itself*. But as these reactions can only take place within narrow limits of conditions, and as the reproduction of the living matter itself is one of the chief functions of these very reactions, it follows that comparatively small variations of the conditions, or of the quality or quantity of the stimuli or pabulum, will produce destruction of the living matter, *i. e.* death and resolution of its elements into the proximate principles. It seems, therefore, to require an effort of the imagination to conceive that the substances which we now easily recognise to be albumen, myosin, and the like, are not the same which we saw a few minutes before in active life, and which we ceased not to watch during the transition. And it seems to require no effort of the imagination to conceive that, if they be the same, some essence, principle, or influence must now be lost, which enabled them to resist all their natural affinities, and gave them totally new powers. But does not the argument from imagination tell really on the other side? Is it not simply in accordance with common sense to deny the name of albumen to a substance which answers to none of its tests, and which can do a hundred things of which albumen is notoriously incapable? And does any one now look for the escape of any active principle when a chemical process comes to a stand, as in the days when Phlogiston or the matter of fire was held to be a substance?

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* We shall frequently have occasion to notice the coincidence between the opinions and principles held and taught by Fletcher before 1836, and those beginning to prevail in the present day; but in no case is it so remarkable as with respect to the doctrines of Dr. Lionel S. Beale, who, by his histological and physiological discoveries, has realised several of the hypotheses of Fletcher in a surprising manner, as we shall see. In Beale's *Todd and Bowman*, 1866, we find, "It is, perhaps, as impossible to conceive a *living chemical compound* as it is to conceive a *living elementary atom*" (p. 103). "The chemical composition of living matter is unknown, and probably unknowable, for it is impossible to analyse matter that is living. To assert, therefore, that living matter is 'protein' or 'albumen,' is to assert that which never has been and never can be proved, and all arguments based upon such assertions must be discarded." (P. 65, Beale *On Life Theories and Religious Thought*, 1871).

§ 69. The distinction of the state of the living matter from that of the proximate principles is so important, that at the risk of repetition some further statement of Fletcher's views must be given in addition to what was said at § § 63 and 64. It might be said there is nothing peculiar in them, as it was often said that organic chemistry was very defective, and that many of the so-called principles were not educts but products, and, therefore, gave no true information as to the state of combination in the living body. This is certainly true in a sense, but by none was the distinction made so absolute, and above all, was it held, as by him, to be the sole cause of vitality. The greater number, as Chaptal, Prout, Thomson, Murray, Henry, Mayo, &c., held that chemical laws were still in force, although guided to produce compounds impossible out of the body, by the "vital principle," "organic agent," "superior agent," "directing principle," &c. And Henry expressly states that in dead animal matter the proximate principles are in a state identical with that in which they exist in the living structure. Against these views Fletcher argues that a power of such a nature must be entirely *sui generis*, and asks, "Is it conceivable that a power confessedly *different* from common chemical affinity should, while it continues to exercise a paramount influence on any substance, maintain it in a state *identical* with that of a common chemical compound." (p. 135.)

Among other expressions respecting the nature of vital affinity, he says, "it does not combine them [the elements] at all in the chemical sense of the word, but merely holds them associated together in an ever varying form of existence, of which the chemists seem to have no idea"* (p.135).

He thinks that "the process of secretion, by which the ultimate ingredients of all vegetable and animal compounds, whatever they may be, are brought together, is, perhaps, an infinitely more subtle and searching power than that of common chemical affinity; and that there exists between the two only a very faint and remote analogy." (p. 140.) Nevertheless, it is not of a nature quite foreign to the properties of matter as the vital-principlists maintain, for he says (p. 136), "that the proximate principles are the results of the combination of certain elements effected by secretion as a process, although not identical with, still analogous to common chemical affinity."

Now, we must remember that the living matter owes its powers entirely to its ingredients, and the mode of combination in which they exist in it; therefore, as different living parts of the same animal and different animals and plants vary in their vital qualities, and even the same part varies at different times, we must expect the proximate principles into which it is resolved at death to vary in proportion and even in number. Likewise, as will be noticed further on, a large proportion of the bulk of organized beings is not really living, but consists of chemical compounds as we find them after death, and we have thus always before us a mixture of dead and living matter; so the problem to be solved by organic chemistry, viz. which are the proximate principles or the chemical compounds if not actually composing it, yet nearest to the living matter? is an extremely difficult and complex one. Accordingly, we find great difference of opinion to prevail among chemists as to the constitution and atomic weights of the albuminous matter.

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* "Of the relations which these elements bear to one another in the living matter, we know indeed nothing; but since every kind of living matter exhibits the same characters, it seems probable that during this temporary living state the elements do not exist in a state of ordinary chemical combination at all. Their ordinary attractions or affinities seem to be suspended for the time." (Beale's *Todd and Bowman*, 1866).

Nor is the analysis of any organ, tissue, or part, anything like complete, and we always find a percentage of "extractives" and other little known matters. Great as has been the progress of organic chemistry since Fletcher's time in 1836, not the slightest step has been made towards filling up the gap between the chemical proximate principles and the living mattter. A glance at the chief points in the present state of organic chemistry will illustrate this and speak strongly in favour of Fletcher's position.

§ 70. "By the term proximate principle is to be understood a vegetable or animal compound formed by the union of certain elements, characterised by distinct chemical properties, and by its union again with others contributing to form the several tissues and fluids of organized beings." By this definition we must be prepared to admit, and afterwards we shall admit, that a great portion of the actual substances composing the body while living are merely chemical compounds, hence the question comes to be whether there are any histological elements whose composition is such that life may be manifested in them while still cognisable as chemical compounds? We may, therefore, pass by the great bulk of binary and ternary compounds with the remark that they are certainly what they seem, and, although not much in that direction has been achieved by art as yet, we can conceive it possible that they may be all compounded in the laboratory chiefly by oxidation from the more complex organic bodies from which they are derived in the living being, although by a very different process indeed.

Some also may now be built up synthetically in the laboratory from the ultimate elements themselves, and, in addition to stearin and urea, which were made in this way before 1836, we have now cyanide of potassium from charcoal, nitrogen, and potassa, and this, when decomposed by water, yields ammonia. Allantoin, formic, oxalic, glycocolic and many organic acids, and various other substances generally derived from the organic kingdom, have been produced artificially from the inorganic elements. And the numbers that have been produced artificially from other organic bodies is so great that it would be tedious to enumerate them. In respect to these, however, it is remarked by chemical physiologists at the present day that they all belong to the secondary organic compounds, such as urea, uric acid, hippuric acid, &c., which are pretty far down in the scale of destruction by oxidation of more complex matters. It is also remarked that in the long list of intermediate products which intervene in the living animal between its complex food and the simple excreta, viz., carbonic acid, water, and urea, the composition of the majority of these is only very imperfectly known even now, and, in fact, can only be stated as known with tolerable certainty as they approach to the simple excreta. We may thus pass over the secondary organic compounds, such as the long list of fatty acids in the vegetable and animal kingdoms: the glycocolic, lactic, oxalic, and other acids, the whole carbo-hydrates—grape and milk sugar, inosit, and glycogen—the neutral fats, the amid substances, as urea, glycin, leucin, cystin, tannin, xanthin, kreatin, and the pigments. All these are derived from the far more complicated group which may alone fix our attention in this question, viz., the albuminates, comprising albumen, globulin, fibrin, and myosin (with syntonin); the albumenoids, namely, mucin, gelatin, chondrin, keratin, elastin, fibrin, protagon, hæmoglobin, and the ferments.

With respect to this last group in particular the observation applies that their chemical composition is only imperfectly known and their constitution or the arrangement of their constituent atoms is wholly unknown. Likewise the very existence of many as distinct chemical substances is not settled, as they are mostly uncrystallisable and hardly to be obtained in a pure state. On this point Dr. Beale remarks, "it is doubtful if many of the substances which have received special names, as albumen, fibrin, and the like are really definite chemical substances of fixed composition" (*Todd and Bowman*, p. 8.) In illustration of this we may remark that Fletcher continually speaks of the proximate principles *par excellence*, as sodo-albumen, ozmazome, gelatin, and fibrin. We have at this day a very different list, viz., that given above, and sodo-albumen is now only represented by the casein of the serum which occurs only in the blood and the spinal fluid. Osmazome has now disappeared from the list, and its very name does not occur in the index to the best works on physiological chemistry. It is nothing but a mixture of several educts and products of animal flesh. The once famous organic radical of the albuminates, protein, is now no longer heard of. The cerebrin, cerebric acid, lecithin, and the phosphorated fats, which for long were said to be the proximate principles of the brain, are said now to be all merely chemical derivatives of protagon. The composition of the albuminates as to per-centage of the ultimate elements is almost exactly the same, and, although they differ much in properties, yet they yield many similar products to chemical processes, and on the whole the tendency of the elements of organic bodies to assume new combinations is very great, showing that the affinity holding together these complex substances is only feeble. For example, the serum-albumen digested with hydrochloric acid becomes converted into syntonin, and with potash into casein. Fibrin digested with hydrochloric acid yields syntonin, which is soluble, but you cannot get back the fibrin from that or any other solution. The probability, therefore, is that we have not yet heard the last word of chemistry on the proximate principles, and that new ones will in future be added to, or take the place of, the old. Does not all this point to the probability that the chemical proximate principles are, as it were, chemical compounds in the nascent state or the earliest on the border line from a totally different state, viz., the vital or metabolic? Now, as the solids and fluids, slowly formed by the vital action of nutrition and secretion, may be looked on as chemical compounds, we must look towards the structureless products of comparatively sudden death as the most likely source of information respecting the actually proximate principles. Among these some spontaneously undergo changes palpable to our senses at or soon after death, the most notable of which is the coagulation of the blood and stiffening of the muscles, which have attracted attention from the earliest times. Let us, therefore, test this question by going into some detail respecting the nature of the fibrin and the myosin which are the efficient agents in those phenomena.

§ 71. In opposition to the opinions almost universally prevalent at the time, Fletcher considered "the coagulation of blood a purely chemical phenomenon under all circumstances" (p. 44), and that "its non-coagulation, while still circulating, is obviously to be ascribed to the perpetual molecular changes which it is undergoing, and which are incompatible with the full development of that principle on which its coagulation depends" (p. 114). That fibrin, being a merely chemical compound, did not exist as a constituent of the living matter, and that all the then (and even still somewhat) prevalent theories of its organisable nature were false and the appearances leading to such conclusions deceptive. He did not, of course, anticipate without experiment, the exact state of the facts now known, but he held the *liquor sanguinis* to be a non-vital chemical mixture, containing fibrin in some state in which it was never "sufficiently long identical" owing to the perpetual and very rapid vital changes going on in the capillaries to allow it to coagulate.

It is interesting to notice that in 1845 the now illustrious J. R. Mayer gave a somewhat similar explanation of the resistance to chemical change displayed by easily decomposed fluids surrounded by living tissues. It is not from any influence of a supposed vital force, he says, but is owing to the constant changes produced by secretion and absorption. (Mechanik der Wärme, p. 79.)

With these plainly expressed views of a strong vitalist like Fletcher, in 1836, before us it is difficult to understand the air of triumph with which Bence Jones brings forward the actual experiment, in his *Lectures on Force* (1868), showing the formation of fibrin from its chemical elements, and commenting on it as a sure sign that the reign of pure chemicalism is at hand. But let us go into the subject in detail.

§ 72. It was found by J. Müller that mixing fresh frog's blood with a weak solution of sugar retarded the coagulation so much that the *plasma* or *liquor sanguinis* could be filtered in a fluid state free from the corpuscles, after which it coagulated as usual. Kühne regards this as simplifying the question by cutting off all the supposed influence of the globules. We have now a fluid in the somewhat paradoxical condition of containing a substance which has the singular property of separating itself spontaneously. The plasma is far from being a saturated solution, especially as regards the fibrin, which amounts to little more than 1 per cent., and which it can never dissolve again. Light was thrown on the subject in 1845 by the discovery by Dr. A. Buchanan of the "coagulant power" of some of the constituents of the blood on the fluid of hydrocele, and the hypothesis began to prevail, especially through the influence of Virchow, that no fibrin existed in the solution, but only a fibrino-genic substance; and finally Dr. A. Schmidt demonstrated that such was the fact, and that fibrin does not pre-exist in the blood as such, but is formed by the union of two chemical constituents of that fluid. It is, therefore, strictly a chemical compound, and this view has been universally adopted. The discovery of the fibrinogen and the paraglobulin as the generators of fibrin cleared up some of the most puzzling facts noted by the older chemists, more especially those observed by Denis of the action of the *Sulphate* and the *Muriate of Soda* on fresh blood. And now the chemical process of formation can be performed by extracting the two constituents separately and mixing them; and the product differs in no way from natural fibrin. But the details of the process interest us as bearing on the influence of vitality. The question of why the plasma coagulates being answered, there remains that of why it does *not* coagulate in the living body, and the important point to know first is whether the same causes which prevent natural coagulation in the fresh drawn blood act equally on the artificial process. The facts on this head have been brought together by Kühne (*Physiol. Chemie*, p. 171). We must recollect that the fresh drawn blood, besides the mere chemical principles, contains living white corpuscles, which, in spite of the opinion above given of Kühne, are held by Beale to have an influence on the formation of, or in fact do form, fibrinogen.

The effects of temperature.—The plasma may be frozen fast, and when thawed is still fluid. It appears even that a temperature of about 32° F. may keep the plasma fluid for an indefinite time. But with increase of temperature the time necessary for coagulation steadily diminishes, and at ordinary blood-heat and a little above it takes place very rapidly. Now the effect of cold in suspending vital action without destroying it is well known, but exactly the same effects of temperature are met with in the artificial formation of fibrin from fibrinogen and paraglobulin. The effect of acids, alkalies, and neutral salts in retarding or favouring coagulation is exactly the same with the natural blood and the artificial mixture. The retardation by *Carbonic acid* and the promotion by the access of oxygen follow the same rule, and this last is important, considering the powerful action usually attributed to oxygen in vital processes. The proofs that the formation of fibrin is chemical and not vital being complete, there still remains much difficulty in understanding the nature of the process. For a quite disproportionally small quantity of paraglobulin is required to convert a large quantity of fibrinogen into fibrin. The two are extremely like each other in chemical composition and in their reactions. It was remarked by Schmidt that both are alkaline in their separate solutions, but on their union the serum becomes more alkaline, as if they had combined with a certain quantity of alkali like weak acids, and the formation of fibrin thus resembles the precipitation of *Silicate of Alumina* from the solution of *Aluminate* and *Silicate of Potash.* Fibrin does not exist in the living organism in health, though easily produced by injuries and diseases, and an important use of it is to limit hæmorrhage where vessels are divided: and besides, according to Beale, "it forms, when effused in internal parts or on the surface of wounds, a temporary tissue, a cementing or protecting substance, or bond of union between separated parts which serves as a nidus for the development of the masses of germinal matter which are to take part in the formation of a higher, more elaborate and more durable texture." Blood clots always contain living white corpuscles at first, and when they have appeared to be organised and take on living functions, it is always owing to the growth and development of these masses of living matter.

With respect to the nature of organisable lymph, Fletcher, of course, denies that it consists of mere fibrin deposited by coagulation, but is secreted, *i.e.* formed by a vital process by the adjacent vessels; and while not denying the outgrowth of vessels from the old parts also, he represents it as "containing within itself the germs of its future nerves and vessels, which being developed first in its substance extend gradually to the neighbouring parts and thus connect them with the newly organised mass. The most philosophical view of the subject appears to be to regard organisable lymph as a kind of germ, and its organisation as depending on the same laws as those by which that of the embryo is effected." (Fletcher's *Pathology*, 184.)

It is impossible to go into such a large subject here, but as bearing on the vitality of fibrin the general opinion is in accordance with the above, and although in most examples of inflammatory lymph fibrin and corpuscles occur together, yet it is to the latter alone the vital properties are attributable.* Indeed, inflammatory lymph is divided by Mr. Paget into the fibrinous and corpuscular varieties according to the proportions in which they are mixed. The origin of the corpuscles is not uniform, as in some cases the migration from the blood-vessels is admitted; and in others the formation by growth from the adjacent parts is still looked on as the chief source, and in fact must always be so if we consider the subsequent development of the product.

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* "In the repair of a divided tendon, masses of germinal matter probably result partly from the multiplication of the adjacent germinal matter of the tissue itself, and partly from the white blood-corpuscles which have escaped from the divided vessels, or from masses of germinal matter descended from these." (Beale's *Oxford Lectures.*)

With respect to the origin of the fibrinogen, the following remark by Dr. Sharpey is important, as it shows that no light can be thrown upon its formation by chemistry which the apparent resemblance of the fibrino-plastic agent to a ferment might lead us to expect. "It is not that the latter converts albumen into fibrin, for after a certain amount of fibrin has been coagulated from the venous fluid no farther addition will generate more, although abundance of albumen remains; and again, a given quantity of fibrino-plastin will not coagulate with equal rapidity and intensity any amount of fluid containing fibrinogen. In short, the fibrino-plastic substance seems not to operate as a ferment or by catalysis, but by combining with the other necessary ingredient." (*Quain's Anatomy*, 1867, p. xxxix.)

With respect to the origin of the fibrinogen, Dr. Beale thinks it possible that it is formed by the vital action of the living white corpuscles and the smaller colourless corpuscles allied to them. These gradually diffuse the fibrinogen through the plasma, and during coagulation the red corpuscles are seen to undergo change during which probably the globulin which they contain escapes. (*Todd and Bowman*, p. 123.) This does not fully meet the objection at first stated (p. 130) against the influence of the corpuscles, although in that experiment the globulin may have already escaped and gone through the filter. But in favour of it we have the facts that, *ceteris paribus*, the total blood coagulates more quickly than the plasma deprived of the corpuscles, for although the latter contains enough of fibrinoplastin to convert the whole fibrinogen, yet the change takes place more rapidly in presence of the greater abundance of paraglobulin which the red corpuscles afford, as they are the most richly supplied with that substance of any part of the body. It is also found that the plasma of blood that has been frozen and thawed coagulates even more rapidly than blood containing the corpuscles in its complete state. In this case the colouring matter has escaped into the plasma and with it paraglobulin. (Kühne, p. 222.)

The fibrinogen occurs in the blood, the lymph, the chyle, and all the serous fluids except the cerebro-spinal. It is most abundant in the pericardial fluid and among pathological formation in the fluid of hydrocele. The paraglobulin, which is distinguished from globulin by some chemical signs and which alone is fibrino-plastic, occurs in the corpuscles, plasma, and serum of the blood, the chyle, the cornea, and aqueous humour, and pathologically in the urine and pus.

To finish the subject of their non-combination in the living body it may be noticed that Brücke examined exhaustively all the conditions applicable to the question, and came to the conclusion that the sole cause was the contact with the living vessels or tissues. Even within the larger vessels blood coagulates around foreign bodies introduced into them; and it was finally shown that it does so under all circumstances when the vital actions of the solids upon it were cut off. None of the chemical reasons, such as the rapid destruction of the specific power of the paraglobulin by ozone (never yet shown to exist in the blood), are admitted by Kühne to be satisfactory, and the direct experiments of Magendie, Brown-Séquard, and A. Schmidt, showing the reproduction of the fibrinogen, are in favour of the influence of vital changes being the cause. A. Schmidt's experiment was as follows:

"The heart of a living turtle being separated from the body was allowed, by the continuance of its pulsation, to exhaust itself as far as possible of blood. It was then injected with serum so as to free it entirely from red blood. It was then filled with defibrinated turtle blood; hereupon the heart, which had been motionless when empty of blood, began immediately to pulsate again, and the first portion of blood that flowed out of the artery coagulated immediately. We thus see, firstly, that fibrinogen is again produced in the defibrinated blood by the action of the pulsating heart; secondly, that this is thrown out as fibrin with unusual quickness for turtle blood, as soon as the blood is withdrawn again from the heart; but if the artery is tied up and the blood retained longer in the pulsating heart, it becomes gradually as slow in coagulating as that of ordinary turtle blood. To all appearance the pulsating heart or the contracting muscle destroys again the fibrinogen which it produces." On this Kühne remarks, "The last fact deserves particular attention, for when we think that the fibrinogen is continually produced and again consumed, and that for the separation of it as fibrin a certain time is requisite during which the action of the paraglobulin must continue, so it is only at the death of the vascular walls, when the fibrogen-metamorphosis ceases, that the opportunity is given for any remnant of this body to unite with the other fibrin-generator and form a clot." This is exactly in accordance with the view of Fletcher, who points out the continual and rapid change that the blood is constantly undergoing. In fact, he calculates that the mass of the blood does not retain its identity longer than three minutes and a half. The existence, therefore, of any individual particles of fibrinogen is merely temporary. It agrees also with the relative diffusion-power of the two substances, for in this respect the paraglobulin far surpasses the fibrinogen, which latter does not diffuse towards albumen, hæmaglobin, or paraglobulin. Now, the corpuscles are much more permanent than the fibrinogen, and as there is no fear of its passing into them the rapid change of it prevents coagulation. Nevertheless, although so much has been done we can hardly say that the non-coagulation in the living state has been yet fully explained. This is also the opinion of the chemicalist, Bence Jones, who calls the chemical formation of fibrin an "unfinished discovery," and, oddly enough, answers the above questions very much in the same way as the physiologist, Fletcher. He says, "the fibrinoplastic substance is rapidly acted on by ozone. The fibrinogenic substance also is rapidly changed after it is formed, so that possibly the necessary quantity of these substances is not given the necessary time to act upon one another for the production of coagula" (p. 58).

From all this we have learned that fibrin is a dead chemical compound, derived from two other equally dead chemical compounds, but we have learned nothing of any possible chemical origin of these latter, nor have we advanced one step nearer to any substance which possesses the powers of living matter described in § 67. Nor do we see that fibrin has any better claim to be capable of putting on vital functions or to be more "organisable" than hair, horn, or cuticle. Before proceeding farther with the general question, let us consider the analogous proximate principle of the muscles, viz., myosin, which is supposed to display vital contractility and the response to stimuli.

§ 73. In the same way as with blood action, Fletcher disposes of all theories of muscular action which presumed the existence of fibrin in the living muscle, while at the same time he explained the *rigor mortis* by its first formation. He controverts the theory of Humboldt, Cuvier, Rudolphi, and others, that contraction of muscles during life can be in any degree dependent on coagulation of fibrin, and maintains that none of that principle exists in the living muscle, and it can therefore have no share in any of its functions. At the same time he shows that the theory prevalent since the time of Hunter and Nysten, that the *rigor mortis* is in some way dependent on muscular contraction is erroneous, and that, in fact, that phenomenon really depends on the development for the first time, and speedy coagulation of fibrin in the muscles. His conclusion is, that "the muscles stiffen, owing to the development in them of fibrin, which they did not previously contain. It is the distinguishing property of fibrine to undergo spontaneous coagulation; but no such coagulation takes place in muscles till they are deprived of their vitality, when they rapidly become stiff; and it is to this cause, and not to the contraction of the muscles, that the rigidity of the limbs, which soon succeeds death, and continues till the putrefactive process has commenced, is to be attributed" (p. 130).

At the same time, to meet the difficulty that a purely chemical cause of *rigor mortis* like this should be prevented by causes operating vitally, such as violent stimulation and over driving, he says that can only be referred to a "deterioration of the muscular tissue, which is resolved into fibrin after death, only when in a state of integrity." These propositions he established by a train of argument unnecessary to reproduce, but which it will be more interesting to illustrate by a short review of the progress of discovery on the subject since.

§ 74. In 1836 the chief constituent of the muscles was universally considered to be identical with the fibrin of the blood, but some years afterwards Liebig finding its reactions different, called it muscular fibrin or syntonin. But since then it has been found that this is not the first product of the muscular plasma on coagulation, and that it differs from that in several particulars. Syntonin is formed by dissolving the coagulum in muriatic acid, but that solution does not coagulate on boiling; it does not dissolve in solutions of common salt; it does not decompose the peroxide of hydrogen, which property is possessed in a marked degree, not only by myosin, but by fibrin and its two generators; it can also be produced from other albuminous bodies, such as fibrin, casein, white of egg, and, as a matter of fact, all the albuminous bodies of muscle can be changed into syntonin, and dissolved by dilute hydrochloric acid. In short, the syntonin is nothing but a product of the action of dilute hydrochloric acid on myosin and other albuminates, which latter are thereby changed into it, and cannot be got back again from the solution, so it does not pre-exist even in dead muscle, and we may dismiss it at once from the proximate principles. The knowledge of its precursor myosin we owe to the indefatigable physiologist Kühne, who gives the first complete account of it, and names it in his *Untersuchungen über das Protoplasma*, 1864. The question of the myosin is more complicated than that of the fibrin, because the muscular plasma from which it spontaneously separates by coagulation cannot be obtained except under conditions which also retain the vitality of the parts, and as yet no chemical generators of it have been found from which it can be formed purely chemically. It is treated of by Kühne as the characteristic contituent of all the contractile substances, both animal and vegetable, which are distinguished by the power of spontaneous coagulation after the cessation of their movements, and which alone in nature contain albuminous compounds, which coagulate at the comparatively low temperature of 35° to 50° Cent. To such substances he, with most of the German school, restricts the term protoplasma. These are the whole substance of many infusoria, such as amœbæ, the soft parts of the rhizopods, the contents of many vegetable cells ; the white corpuscles of the blood, lymph, and chyle ; the corpuscles of connective tissue of mucus, saliva, pus, &c., all which are capable of apparently spontaneous movement, also the fluid contents of the sarcolemma or muscular plasma.*

Dr. Beale's more general application of the term protoplasm or germinal matter to the living matter *par excellence* will be followed here. The substance from which the myosin spontaneously separates is the single-refracting matter contained in the sarcolemma in its purest state, but it is very difficult to obtain except by methods which also retain vitality. These are drying and cold. The muscle of the frog may be quickly dried before coagulation, and the muscular substance from the dried portion may be dissolved out, and will then coagulate spontaneously. This does not happen with blood or lymph. Now, we know that many low forms of living animals, such as the rotifers, may be dried for a long time without destruction of their vitality. The other method is by cold, which we know also preserves the vitality in a state of suspended animation, and many animals, fish especially, may be frozen and restored to life afterwards. So we must notice that in the following processes we must for a time not only get the precursor of myosin, but some part of the matters operated on must be in the living state. Kühne takes the muscles of frogs, carefully freed from blood, by injecting a ½ per cent. solution of common salt till it returns colourless by the veins. They are then submitted to various manipulations at a temperature of—7° C. when hard frozen, and finally a fluid is obtained on thawing, which can be filtered, and will then display the properties of the muscular plasma. He has found that uninjured muscles exposed to a cold of—7° to—10° Cent. for three hours, when thawed in a room at 15° C. (64° F.), remain irritable on an average for six hours, so we may presume that these fluids retained their normal properties. The muscular plasma thus obtained is a yellowish, syrupy, alkaline liquid, but it is difficult to free it from portions of fibrous matter, and it quickly chokes the pores of the filters. It is therefore quite possible, although Kühne does not advert to the question, that the finer particles of living matter, as spoken of by Beale in the blood plasma, may be present, and we cannot say what part they may play in the subsequent change that takes place. That change is spontaneous coagulation and separation of the myosin, which is analogous to fibrin, although differing in some particulars in its reaction with acids, alkalies, and salts. The formation of myosin takes place very slowly at 0° Cent., but almost instantly at 40° Cent. It is also produced immediately by dilution with pure cold water, so it can be got pure by dropping into very cold water, when each drop falls to the bottom in the form of a firm elastic ball of myosin. If dropped into a 0·1 per cent. solution of hydrochloric acid it coagulates at first, but is again dissolved before it reaches the bottom, but it is now no longer myosin, it is converted into syntonin.

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* Dr. Beale says (*Protoplasm*, p. 15) that Kuhne considers all contractile material to be protoplasm, and that he applies that term "to the contracting muscular tissue which exhibits structure, as well as to the structureless." Now I think that it is scarcely a correct description of Kühne's statement that a substance which after death becomes myosin is contained in the single-refracting contents of muscular fibres, and in all other organized substances which exhibit movements. That is simply a statement as to facts, whatever be the cause of those movements.

To follow now the changes that take place in the death of muscles, we will take for granted that the above plasma is really the fluid part of the contents of the sarcolemma, which is not a solid fibre at all, as already stated at § 23 (p. 52). The first step is the gradual disappearance of the contractility, so that the strongest stimuli produce no longer any shortening; then, 2nd, appears the stiffness shown by loss of flexibility and elasticity; 3rd, the acid reaction; 4th, the opacity, now the *rigor mortis* is at its height. When these are contrasted with the state of the contracted living muscle, the difference is plain. The latter is transparent, elastic, soft and alkaline generally, and if the sarcolemma be torn, the contents are pressed out; this last does not happen in the death rigidity. After going through all the phenomena and conditions of the state, Kühne concludes that the *rigor mortis* depends on the development and coagulation of the myosin from the plasma, which is fluid in the living state. He says (*Physiol. Chemie.*, p. 282), "An end was put, by the reflections of Brücke, to all former views which looked on the death-contracted muscle as eminently living, in as far as it was thought to be tetanically contracted."

Hermann also (*Grundriss der Physiol.*, p. 220) says, "The circumstance that a shortening of the muscles took place in *rigor mortis*, as in their ordinary activity, nourished false views on the nature of the stiffness for a long time. It is only since the supposition (Brücke) of, and the proof (Kühne) of, a coagulable body in the muscle that the doctrine here given has become generally taught."

These authorities seem not to be aware that the arguments of Fletcher were quite as cogent long before.

The muscles do not stand alone as to stiffening at death, for a certain amount of that takes place in many tissues, owing to the protoplasm they contain. In all these contractility and the spontaneous movements cease, and coagulation takes place at a temperature between 35° and 50° Cent. (78° and 112° F.). Kühne considers it probable that all varieties of the contractile protoplasm occurring as part of the higher animals, and when it forms the whole animal, as in the amœbæ, &c., contain myosin. It is also found in the axis cylinders of the nerves and in pus. We have thus a substance that comes very near to the living matter, while at the same time we see undoubted proofs that it does not exist till after death. But it is even doubtful whether its immediate precursor in the muscular plasma is not also a mere chemical compound, like the fibrinogen, so it may not stand next to the living matter after all. Is the myosin, in fact, like the fibrin, a compound of two generators? In favour of this supposition Kühne gives several chemical reasons which need not be repeated, as they are not conclusive, and certainly these substances, if they exist, have not yet been isolated. The history of the muscular plasma is not finished with the separation of the myosin, for the muscle-serum, which remains, still contains albuminous bodies of interest. At temperatures above 0° Cent. it soon becomes acid, and at 45° Cent. again deposits a coagulum, which is not myosin. After filtration there still remain at least two other albuminous bodies, viz., the potash-albuminate or casein, and the serum-albumen, which is identical with that of the blood. Besides these, it contains the usual salts, the phosphate and lactate of potash. All this gives us very little information as to the state of the muscle-plasma before the formation of myosin, and while it is either itself in the living state, or mixed with living matter in the process of extraction.

As yet we have seen no substance capable of responding to the action of stimuli, nor seen reason to suppose that any part of the function of force-extrication can be chemical rather than vital or metabolic. Nevertheless, there are physiological grounds for supposing that a large part of the process may be physical and chemical. It is too large a subject to go into here, but we may with advantage touch on some of the points which show how far chemical explanations can go, and where the different order of things we call vital or metabolic must come in, to all appearance. All agree that the mechanism of muscular motion is purely physical, in as far as it is an approach or change of direction of material parts, just as shortening would be produced by a similar change in any similar thing out of living beings. Fletcher, in reviewing the theories of the day, concluded that the most probable of them was the zigzag bending of the muscular fibres. Now, since the solid fibres are abolished, the shortening is considered to be produced in what is still a similar way, viz. by the changes of position of the disdiaclasts,* which are the component parts of the muscular prisms, rows of which, enclosed in sheaths with the plasma, constitute the muscular fibres. The source of the force producing these movements is, without question, that of chemical attraction set free by the satisfaction of stronger affinities from complex substances passing down to simpler combinations. So far we may speak with tolerable certainty, but it is believed on good grounds that direct oxidation is not the chemical change which sets force free; and also there is a complete gap in our knowledge between the origin of the force and its application; electricity is held by some good authorities to be the intermediate agent, but nothing is positively known, except that it is certain that it is not heat which is converted into work (Voit, Clausius, Fick).

Here we must be excused for going farther into details, for we stand now on the boundary line between vital and chemical action, for contractility has always been considered one of the cardinal attributes of life alone. Now, since the discovery of the law of conservation of force, we know that there can be no such thing as a property of contractility in the sense of a capability of producing motion without expenditure of an equivalent of force whether the matter be living or dead; and that in the living matter there is no other source of force than that of chemical attraction set free as said above. There is no doubt this doctrine has tended much to the revival of purely chemical theories of life, but without reason in my opinion, for it is never pretended that vital affinity could create force any more than chemical affinity, both being merely properties (§§ 60, 61, 64); and on any analytic change of molecular aggregation the amount of the outcome of force depends solely on the less amount of potential energy in the new compound than in the original one. Hence it makes no difference whether that change takes place while the atoms are under the influence of vital or of chemical affinity as far as the amount is concerned. The problem set before us in muscular force-extrication is this: we have a complex unstable substance with high potential energy, viz. blood, and oxygen likewise with high potential energy in the capillary vessels at one end of the scale; while at the other in the muscle we find products far down in the scale of potential energy or totally spent; viz. lactic and carbonic acids, while the difference of force is set free in the forms of work and heat.

* Brücke, in Stricker's *Histology*, i, p. 240.

Whether the intermediate process be chemical or vital, or partly both, can make no difference in the quantity of force set free. It is, perhaps, most likely that the whole process takes place within the inscrutable domain of the metabolic affinity, in which case all the chemical products—both those existing in the muscles during life, and those which only come into existence after death—would spring directly from the living matter.

We have an example of decomposition of pabulum by purely vital action with evolution of actual energy as heat in the growth of the yeast-cell. This is a simpler process, as oxygen takes no part in it, and there is no mechanical motion produced; so, in view of the greater complication and need of more rapid action, it may be that the evolution of force proceeds from a chemical compound formed by the living matter—a secretion, as it were, containing the oxygen synthetically combined, and ready to split up with evolution of force.

In this case the myosin, &c., may have chemical progenitors. Such are presumed in the chemical hypothesis of Hermann, given at § 23 (p. 51), of which a few words more may be said. The hypothetical substance "inogen" there spoken of, whose splitting up into myosin and the spent products carbonic acid and lactic acids, is presumed to furnish the force extricated as heat or work, cannot well be like either of the fibrin-generators, because the myosin in Hermann's theory plays the part of a chemical ferment, causing the oxygen and certain non-nitrogenous matters of the blood to unite into the form of inogen and immediately decompose, while it returns to the state of myosin, just as sulpho-vinic acid is supposed to be the intermediary in the catalytic formation of ether by sulphuric acid and alcohol. Now, we know that is not the case with the generators of fibrin, which simply combine, although in unequal proportions, and neither can be got back from the product. We have, however, no example in chemistry of such a complicated process as the inogen formation, and certainly not, as before said, § 23, under stimuli; and the supposition of a myosin formation at all before death goes against what has been said of the cause of death rigor, and brings back Hermann's theory to that of Humboldt and Cuvier. Hermann, indeed, is obliged to suppose that in natural muscular action the myosin does not really coagulate, but remains in a first or nascent stage, in which it is merely gelatinous, and not optically demonstrable (p. 226); and from this it may pass back over and over again into "inogen" like a ferment. As one point in favour of this first stage of myosin formation being a part of natural contraction is the fact alleged on Hermann's own authority, that "a muscle cut out of the living animal produces the same total amount of carbonic acid, whether it directly stiffens into death or previously has been excited to contractions, with formation of carbonic acid; thus the more carbonic acid there is produced by contraction, the less is produced by stiffening" (p. 225).

The same has been observed by Ranke with respect to the lactic acid. These are certainly in favour of the theory that some substance is formed as long as the muscle lives, whose splitting up produces carbonic and lactic acids, and thereby extricates force, either as heat or work, but they do not prove myosin to be thus formed.

One thing stands fast, viz. that no process of direct oxidation takes place during muscular action. This has been settled by the elaborate experiments of Voit, who shows on the large scale that, although the source of muscular force may be the formation of the non-nitrogenous products derived from fats and sugars, yet the albuminates must take an essential part in the process, "with their important function of condensation of oxygen, without which no combustion of fat and no extrication of force is thinkable; hence the quantity of the albuminous matters determines, also, in this case the amount of the work."* The formation of some substance by oxidative synthesis is, therefore, a necessary preliminary to force extrication. Now, it seems not improbable that the splitting up of some merely chemical compound of high potential energy is the immediate source of the work and heat of muscles, but, as before said, I cannot but believe that we have now got to the limit of chemical action, and the formation of that substance is the work of metabolic or vital action.

Hermann says that the splitting up of his "inogen" is prevented by sudden strong heat or mineral acids, but the substance is at the same time destroyed. This is in allusion to the fact that by throwing a portion of living muscle into boiling water or by strong mineral acids it loses the power of forming carbonic acid, it does not become acid, nor does it stiffen. We have here the counterpart of Fletcher's statement that the non-stiffening from excessive stimulation is owing to *deterioration* of the living matter and its too rapid death to form the products of its gradual death. We must remember that our knowledge of what exists in the plasma before death is merely speculative. We know that in the contraction of muscle carbonic and lactic (properly paralactic) acids are produced, but from what matters they proceed we do not know. There are likewise a great variety of extractives, but it is not known in what quantities they exist in the living state, and although Voit proves that the use attributed by Liebig to the kreatin is exaggerated, if not quite erroneous, still it is a defect in Hermann's or any similar theory that nothing is said of them at all. I am not unwilling, as a vitalist, to admit the partial application of some chemical process as above, but I think all will agree in Hermann's own conclusion respecting chemical theories as applied to the whole process.

"There is as yet entirely wanting the more profound knowledge of muscular action, especially of the connection between change of form, heat formation, and variation of electric currents among each other, and with the chemical splitting-up processes. All theories hitherto put forth are faulty" (p. 240).

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* *Ueber die Entwickelung der Lehre von der Quelle der Muskelkraft, &c.* von Carl Voit, p. 14.

At any rate, by the experiments of Pettenkofer and Voit we have got rid of the coarse chemical notion of combustion of the fats and sugars, or the muscular tissue itself, producing heat which is again converted into work as in a steam-engine.

We may, indeed, take Voit's statement of the essentiality of albuminous matters for the combustion of fats and sugars simply to mean that such must be performed by a vital process, for such processes are only displayed by compounds into which nitrogen enters, and therefore a due proportion of nitrogen must enter into the diet. To enter into the process by which the fixed proportion of nitrogen is retained in the system belongs to the question of general nutrition, and, although bearing on the question of muscular action, would lead us too far from our subject to be entered upon here.

The action of a number of chemical substances as stimuli in inducing contraction and coagulation on application to the cut ends of muscular fibres and not to their nerves, seems to Kühne to show that the whole process is chemical. This will be considered under the general action of stimuli; but, in the mean time, it is not difficult to suppose that many chemical substances would cause the splitting-up of a supposed "inogen," and that the living matter might, under stimulus of the nerves, secrete some ferment which might have a like effect. On the other hand, it is also quite as conceivable that all those agents act on the living matter, and that the myosin coagulum which appears on their operation is the result of over-stimulation and death. Myosin may, no doubt, be formed and removed even in the living state just as fibrin, but then it is a pathological product. The insuperable difficulty for the chemist lies in the *formation* of all those substances engaged in the work of contraction which do not exist in the blood, which is the sole source whence they are derived, not to speak of the formation of the muscular apparatus itself.

We have already said that Hermann's hypothetical inogen has not been isolated, because it splits up by every chemical treatment, and in another place (p. 211), after enumerating the chemical constituents of the dead and rigid muscle, he says, "As the process of coagulation as well as the contraction is accompanied with changes in it which are still unknown, and as the uncoagulated muscle-plasma cannot be analysed without producing contraction or coagulation, so the above-named constituents of the coagulated muscle cannot be looked on as the constituents of unaltered living muscle."

It is superfluous to enter on the chemical history of albumen, gelatin, protagon, or any more of the proximate principles, as it must be sufficiently obvious that, in spite of the advance of organic chemistry, its stand-point in respect to Fletcher's theory of life is exactly the same now as it was in his day. To return to the more general question.

§ 75. In no substance chemically definable have we yet seen any true attributes of life, and many of the chemical school will grant that those we do know are products of the chemical processes used, or of changes that have taken place before analysis could be begun. But these persons mostly imply that in time, by increased skill, we shall at last get to some ultimate substance really pre-existing in the living matter which shall be capable of vital functions and at the same time cognisable by chemical reactions. Also which by combination and splitting up shall be capable of forming the long and complicated series of substances forming the tissues and fluids by whose really physical and chemical properties a great number of the subordinate functions of living beings are performed. Liebig, for example, in his recent papers on fermentation, after pointing out that a number of important processes (such as the primary stages of digestion) are really the work of purely chemical ferments, goes on to say that these ferments may possibly be formed by the chemical reactions of other unknown compounds; and thus by a long series of combinations which chemistry will one day reveal to us we may be able to explain the formation of the tissues and the functions of life without having recourse to what he sneers at as the antiquated notion of vitality or vital force, as he sometimes calls it, as if the two expressions meant the same thing. Vitality is, however, a property and not a force—two very different things, as it has been attempted to be shown at §§ 61, 62.

This by the way; but if such a time is coming it must be very slowly, for we do not seem to have made much progress in the last forty years. We have, it is true, seen the genesis of one supposed simple proximate principle, viz., fibrin; but if we can do nothing more than find two chemical progenitors for each, and even two for each of those again, it would not help us much as long as we cannot separate and recombine them, and combine the different generators with each other, so as to form different substances, all of which are the ordinary details of chemistry. Nay, even if we can so far imitate vital action inasmuch as we can make compounds by processes in which one substance is not consumed, *e. g.*, as in catalysis and chemical fermentations, we must remember that we have not yet made chemically any of the organic ferments such as pepsin, ptyalin, and the like. I shall afterwards consider catalytics under the head of stimuli, but in the mean time it may be said their resemblance to vital agents is only superficial, and they all act by double decomposition and combination, like all other chemical agents; but here it may be said if they remain stationary they do not *grow.* No catalyser or ferment grows or reproduces itself in the process any more than spongy platinum. I am speaking here, of course, only of the chemical ferments such as diastase, emulsin, pepsin, &c., not of the physiological ferments like yeast, which last are vital agents. In short, to meet the difficulties of the first formation of organic substances the chemist has nothing but speculations and hopes with the slenderest foundation of facts as yet established.*

§ 76. The opinion that in the living state matter was withdrawn from chemical affinity, and that the proximate principles did not represent the actual state of combination of the elements in the living body, was not peculiar to Fletcher, but held pretty extensively by physiologists, such as Rudolphi, Barclay, Adelon, Prichard, Tiedemann, and others, and one great chemist Berzelius. From Adelon, Fletcher quotes, " Les molecules qui forment les solides du corps humain, étant associées en vertu d'une affinité spéciale, dite vitale, et que les chimistes n'ont pas en main, comment ces chimistes pourraient ils prétendre faire une analyse de ces solides? Ils ne font que les detruire." But it is to be remarked that the majority of the above belong to the vital-principle school, and that their real meaning probably differed very little from that of Prout, Thomson, Henry, and the others who expressed it as chemical affinity modified by a superior power. There is necessarily much vagueness to be expected in the language and ideas of theorists, who have always a *deus ex machinâ* at their beck and call, like the vital principle, ready to cut every knot they cannot untie; and I cannot find that any one expressed with decision and clearness the hypothesis of Fletcher above given (§ 67, 68, 69). Since the promulgation of the present doctrine of force, however, it must be confessed that the view of an essential and irreconcilable difference between vital and chemical affinity has lost ground, and the great majority at present are inclined to the purely chemical theory of the constitution of organised matter. When we look back on the history of physiology, we observe that at every important discovery in physics and chemistry there has been a recrudescence of physical and chemical theories of vital phenomena, but that after a time vital physiology has surely regained its ground. I am persuaded the same will happen again ere long, and in the mean time, Dr. Beale whose discoveries in histology entitle his word to the highest respect, has stepped forward as the champion of pure vitalism. In the point now under consideration he repeats the words almost of Fletcher and Adelon, denies the existence of albumen or anything like it in living matter, and says the chemist can tell us nothing about the molecular condition or chemical composition of the germinal (living) matter, for the instant he commences his analysis the living matter has ceased to be living matter, and he is merely dealing with the products resulting from its death. (*Oxford Lectures*, 1868.)

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* "The lifeless passes through no definite stages or states of being; the living invariably does so. The lifeless catalytic body does not necessarily alter in chemical composition during its action; the living one is always undergoing change in its active state. The first cannot be said to *form new material,* the last always exhibits this property. If the catalytic platinum could take up and convert the materials around it into *platinum,* and give rise to something differing in composition and properties from itself, as well as from the matters around it, which it had taken up, an analogy would exist between the phenomena above mentioned and catalysis." (Beale, *Medical Times*, 10th March, 1866.)

It has always appeared to me a very singular thing that the vital-principle school should go out of their way to raise the organic elements out of ordinary chemical affinity, because with an agent of such transcendant powers at command it would be quite as easy to perform all the vital functions with albumen, fibrin, &c., as with new substances held together by a new affinity. And if the organic elements are really emancipated from chemical, and subject to a new affinity—and therefore must have new properties which would be presumedly capable of performing new functions impossible to chemical compounds—where is the use of the vital principle? Again, for the more modern school, who can bring themselves to imagine that any mere force—measurable as $m. v.^2$, and correlateable with heat and mechanical motion—will confer upon any substance whatever the distinguishing powers of life, they may just as well accept albumen and the rest as the materials for that force to act upon; which they accordingly do, and accept the ordinary proximate principles, or some other to be soon found, as the actual substances which fulfil vital functions. But in accounting for the striking differences between their conduct in and out of the body, they fall into extreme vagueness as to conditions and modifying circumstances, and if you probe the matter to the bottom you always find that the supposed vital force is nothing else than our old friend the vital principle with a new name. As examples, we find this shown in the expressions of men of the highest merit as experimenters, such as Kühne and Parkes. The former, after describing the movement of the amœbæ, in answer to an objection how it is that "such a small drop of albumen as an amœba" can withstand the gradual diffusion into the water, says, "But the amœba-substance is capable of, at the same time, assimilating nourishment and rejecting useless remains. It thus regenerates itself, and it is this maintenance of equilibrium of bodily substance by assimilation and excretion which we call life; this self-conservation against external influences, this struggle for existence, belongs to the definition of life."*

This last part of the sentence has a teleological ring about it that puts all the pretensions to exact chemical science out of court. People never talk of acids, alkalies, or salts, "struggling for existence." Dr. Parkes, in his recent lectures on the elimination of nitrogen,* says, "If we have no idea why an increment of carbon to a compound of carbon, hydrogen, and oxygen should give it a different consistence, weight, colour, and taste, or the power of crystallization and accretion, how is it possible we can form any conception what arrangement of particles shall evolve contractility, sensation, and the power of assimilation and growth?" This implies that some arrangement of particles does produce vitality, and the passage might have been written by Fletcher, as, indeed, fifty similar passages are written by him. But then Parkes goes on to illustrate the matter thus:—"A particle of an albuminate—a dead particle, as we term it—passes into the body, is attracted by a living structure, and then becomes gifted by the contact with the marvellous power of contractility and sensation." Granting that this is merely an illustration, similar to the vague resemblance, noticed by Owen, between magnetic and vital action, yet he nowhere gives us to understand that albumen is anything but albumen when it takes part in vital changes, or that these are other than chemical. Likewise, he speaks of "the force which absorbs nutriment into the germinal cell, and eventually differentiates it into tissues and organs."

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* *Das Protoplasma*, p. 39.

The idea here is evidently that of Schwann, who says, "We must ascribe to the cell membrane, not only the power in general of chemically altering the substances which it is either in contact with or has imbibed, but also of so separating them that certain substances appear on its inner and others on its outer surface" (p. 199, *Schwann and Schleiden's Researches*). This faculty Schwann then proceeds to trace to something like galvanism. Now, we must recollect he wrote in 1839, before the true nature of force was known, and heat, light, and electricity were held to be imponderable substances, and it was impossible to make the distinction we now do between property, force, and power. From what has been said at § 61, 62, it cannot be admitted that any force, in the proper sense of the term, can have the above power. Apparently, also, Dr. Parkes is not satisfied with the position, for he is obliged to fall back on the darkness of the subject, and concludes by doubting "whether some greater mystery still is not behind the veil, and whether human knowledge will ever suffice to rend it." In this last clause all will, I imagine, agree; but why should vital action of matter be a greater mystery than chemical action, which is already pronounced to be inscrutable? Is not the additional difficulty raised by those who insist that the chemical affinity is still existing, and is controlled, counteracted, and directed by a something called force, whose powers have no parallel in the physical forces of which we are cognisant?

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* *Lancet*, March and April, 1871.

With a similar inconsistency, Cl. Bernard expresses the opinion that some day all the phenomena of living beings will be reduced to physio-chemical considerations, and immediately goes on to say that they are all subordinated to a *force vitale créatrice* or *idée directrice,* whose nature, when we look further, can be nothing else than the vital principle of Barthez, the inventor of the term, but not of the thing, if an imaginary substance can be called such, for it is nothing but the Psyche of Plato. And so we shall find with the theories of all those who assert the purely chemical nature of vital phenomena, and do not perceive the necessity of a distinct and peculiar affinity for their production.

§ 77. As might be expected, Fletcher gives no countenance to the idea of any intermediate stage between the vital and chemical state of matter—any stage, as it were, common to both which would permit the gradation of one into the other. There is no such thing as vito-chemical in the sense of partaking of both states. On the contrary, the division is sharp, abrupt, and absolute, and between them is an unfathomable gulf. This applies equally to the smallest particle of living matter which forms part of organised beings, as between its state while living and possibly an instant afterwards when dead, and to the organic contrasted with the inorganic kingdoms of nature. He quotes with approbation the comment of Dr. Johnson, that there are "infinite vacuities" in the scale of animated creation, wherever it begins or ends. He rejects as altogether false and unfounded Raspail's classification of organic compounds on the principle of "their being either already organised, tending to become organised, undergoing organisation, or organic.". The expression organisable is used frequently in a false and misleading sense, applied especially to the albuminates, as if they were capable as such of taking on vital action. Gavarret (*Phénomènes Physiques de la Vie,* 1869) speaks of "the albumenoids as the true *organisable* substances from which are borrowed the fundamental elements of all organisation," p. 269). The albumenoids are no nearer life than the inorganic materials, so the above cannot be admitted, except in so far as they form a pabulum more easily assimilated by some organised beings. On this subject Dr. Beale is entirely in accord with Fletcher, and his frequently repeated and strong expressions must be familiar to almost every one. I may, however, quote this sentence from the Oxford lectures of 1869 :

"Between the *living* state of matter and its *non-living* state there is an absolute and irreconcilable difference ; that, so far from our being able to demonstrate that the non-living passes by gradations into, or gradually assumes the scale or condition of, the *living,* the transition is sudden and abrupt, and that matter already in the living state may pass into the non-living condition in the same sudden and complete manner."

In other places I think Dr. Beale lays unnecessary stress on the fact that the chemists have never yet succeeded in making anything the least like albumen; so can they ever hope to make a living cell? It is quite true they have not, and it is, no doubt, most probable they never will, but still it is possible. We know that diamonds, jacinths, sapphires, emeralds, and other precious stones, have never been made by the hand of man, but nobody doubts they are merely chemical compounds, and if we shall make them one day we may also make albumen. We can afford to grant that, and yet the purely chemical theory of life would not be advanced one jot. It is more important to notice the argument which is almost universally relied upon by the chemicalist school, viz. that if we can make artificially the organic compounds, the fact is in itself a sufficient proof that the process of their formation in the body was merely chemical and not metabolic. On this point Fletcher observes, "We know, indeed, that binary compounds, such as carbonic acid and water, may result equally well from common chemical affinity as from secretion; and there is nothing, therefore, irrational in the idea that ternary and quaternary compounds might do so likewise" (p. 125). He then simply states the fact of the double origin of some, and possibly of all, the organic compounds, and dismisses with ridicule the inference founded on their chemical origin that we shall ultimately make living beings in the laboratory. But since then we have some direct testimony of the existence of the metabolic state from chemistry itself. We have the evidence of a slight difference in constitution of some chemical substances when resulting from vital action and when prepared in the ordinary way in the laboratory. For example, it was discovered by Pasteur that asparagine possesses the property of rotating the plane of polarisation of polarised light, which is likewise possessed by its derivatives, aspartic and malic acids, which are formed from it by subtracting one and two atoms of ammonia respectively. These two still possess the power of rotation, but the transition from malic to maleic and paramaleic acids causes the rotation-power to cease. The paramaleic acid is also called fumaric, as it is found in fumitory. M. Desaignes discovered that aspartic acid could be made artificially from fumarate of ammonia. Pasteur then tried whether the artificial aspartic acid, *i. e.* that derived from a so-called inactive or non-rotating body, possessed the rotating property like the aspartic acid derived from the active body (asparagine), and found that it did not. Thus, an organic compound, made artificially and apparently differing in no respect in the great bulk of chemical relations, cannot necessarily be assumed to be identical with that derived from the organic kingdom.* This is noticed by Professor Stokes in his address to the British Association, 1869, who says that the statement still remains true that no substance active in the above respect has been formed artificially from substances not possessing that property; therefore, the artificial substances cannot be said to be absolutely identical with the natural ones, *i. e.* those formed through vital processes. He suggests a physical explanation of the fact, but it still remains true, and, as far as it goes, tends to show that the metabolic process is distinct from the ordinary chemical action by which the same substance may also be formed. A second and stronger testimony to the same effect is the extreme difference of the mode and conditions under which a substance capable of being made both naturally and artificially is produced. Of this I have collected numerous instances, but it is unnecessary to accumulate proofs, so I shall confine myself to the instances brought forward by Dr. Beale. After remarking that the chemists are prone to speak of the synthetic actions of plants as only transformations such as they themselves perform in the laboratory, he contrasts the two modes thus:

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* See Biot's report on Pasteur's aspartic and malic acids, *Philosoph. Mag.*, v, 4, p. 275.

> A "cell, or rather transparent structureless matter does, without any apparatus or machinery whatever, and at a lower temperature and in a marvellously small space and with marvellous speed, much that the chemist cannot achieve with all sorts of complex apparatus, space millions of times as great, and time millions of times as long, at his disposal, and communicates its wonderful powers to new matters into the bargain."
>
> "The *plant* may consist of a minute particle of *living*, clear, transparent, structureless, soft, semi-fluid matter, which, under certain conditions, takes up the water and carbonic acid, and the complex organic bodies are formed or form themselves! Now, let us see how the chemist proceeds to form, by the synthesis of its component elements, such a comparatively simple organic substance as acetic acid. Dr. Odling tells us how Kolbe effected the formation of this substance from carbon by a series of inorganic reactions:—'Disulphide of carbon, CS, was first obtained by the combustion of charcoal in sulphur vapour. This compound was acted upon by chlorine at a high temperature, whereby it was converted into chloride of sulphur and chloride of carbon, CCl. Then, by transmission through red-hot tubes, this last product was *transformed*, with evolution of chlorine, into the so-called sesquichloride of carbon, $2CCl = Cl_2 + C_2Cl_2$, and eventually into the so-called bichloride of carbon or tetrachlor-ethylene, $C_2ClCl_2 = C_2Cl_4$.

In the course of his examination of this tetrachlor-ethylene, Kolbe observed that by exposure to chlorine in presence of water it was decomposed into a mixture of hydrochloric and trichlor-acetic acids, thus:

$$\overset{\text{Chlor-ethylene.}}{C_2Cl_4} + \overset{\text{Water.}}{2H_2O} + \overset{\text{Chlorine.}}{Cl_2} = \overset{\text{Chlor-hydric.}}{3HCl} + \overset{\text{Chlor-acetic.}}{C_2HCl_3O_2}$$

Then, by subjecting this trichlor-acetic acid to the action of nascent hydrogen, he successively converted it into dichlor-acetic acid, $C_2H_2Cl_2O_2$, monochlor-acetic acid, $C_2H_3ClO_2$, and finally into normal acetic acid, $C_2H_4O_2$.' What can be more conclusive of the presence of some power in the plant, in obedience to which the elements combine with one another to produce the required compound? The highly complex substance is formed very quickly under conditions so different from those necessary for its production in the laboratory as to render it certain that the synthetic processes must be effected in a totally different manner in the two cases. I should have concluded that the chemist would have been led to reflect upon the wonderful and mysterious agency at work in the simple living matter of the plant, by which the same compounds are produced in a manner *so totally distinct from* that in which he is able to produce them. When the chemist has succeeded in forming a little clear transparent stuff like that in the plant, which will take up water and carbonic acid and *transform* organic compounds, it will be quite time enough for him to call the plant a 'machine' or a laboratory; and talking of the 'fiction of vital force' and *artificial processes* of oxidation 'more or less similar to the *natural processes* taking place in the animal body,' ought we not, for 'more or less similar to,' to substitute '*totally different from*'?" (*Medical Times*, 1866, p. 362.)

"How are we to account for the fact of distinct classes of substances being separated, or separated and altered, by the agency of one and the same cell or nucleus? The oily, saccharine, and albuminous constituents (butter, sugar, caseine) of milk have not been discovered in the blood, and there is only one kind of cell to form these three classes of substances. Does the same cell-wall or nucleus separate from the blood, at the same time, oily, starchy, or saccharine and albuminous matters, and convert these into the particular constituents of milk, or are they separated one after the other?" (*Beale's Todd and Bowman*, p. 109.)

One would think that such examples were sufficient to show the existence of some radical distinction in the two processes, but Professor Stokes, in allusion to similar facts, still asserts that the natural organic compounds were "formed by the play of ordinary chemical affinity, not necessarily nor probably by the same series of reactions by which they were formed in the laboratory, where a high temperature is commonly employed, but still by chemical reactions of some kind." As if those words of simple assertion could explain away the insuperable difficulty given even by temperature alone.

Professor Wyville Thompson follows in the same strain when he says, "It may be fairly questioned whether the chemical relations of the component elements of an organised body are in any way directly affected or controlled by life. It has become quite conceivable that a constant adjustment and readjustment of membranes and colloid diaphragms, in the presence of powerful catalytic agents, may possibly explain the maintenance of almost any chemical conditions, however complicated." (*Nature*, 25th May, 1871.) He ought to go a little more into detail. Are we to understand that a portion of clear structureless stuff, $\frac{1}{50000}$ of an inch in diameter, such as, say, any part of the secreting protoplasm of the liver, is provided with all the above complicated apparatus to enable it to secrete from the blood the complex fluid bile, not one of whose constituents pre-exist in the blood? Surely Beale's complaint is most apposite here, viz. why do the chemical school always fall back on the very complicated structures of complete individuals high in the scale as a base for theories? Again, what analogy is there between the mechanical catalysis, as we may call it, of spongy platinum, which merely brings oxygen within molecular distance, and the above? Or even between it and chemical catalysis, as in the manufacture of chlorine by sulphate of copper and hydrochloric acid, where the action is simply that of definite chemical combination through a definite intermediate compound, or if the latter is not actually formed, the definite affinities tending to it are engaged? Compare that with the extreme complexity of the metabolic action of the living matter of a milk or bile cell, which not only transforms the pabulum into such a variety of matters at one moment, but makes and renews itself. This last power the author admits to be inexplicable by chemistry; why, therefore, attempt to glide over the immense distinction between the metabolic or vital and any known chemical action by stretching the already vague word catalysis beyond any signification warranted by facts?

Now, the tendency of this is not merely curious or speculative, but is in the highest degree practical; for the question can never be of the action of the causes of disease or of remedies in a chemical sense, nor can we speak of parts, organs, or tissues, which are more or less organised, and to a greater or less degree susceptible of being acted on chemically. On the contrary, the whole living being must be divided into two parts, sharply and distinctly defined—one which is wholly chemical, and the other wholly vital, while, in respect to the latter, no knowledge of the chemical properties of any substance can give us the slightest clue to the behaviour of the living matter exposed to its influence. Hence the futility of all chemical theories of pathology and therapeutics noticed in § 64, except in the very limited sphere in which we can act on the merely chemical part, without at the same time exposing the vital part to the same agent. This subject is too large to be even touched upon at present, so we may pass to the extremely interesting question whether it is possible, as yet, to point out anatomically that sharp dividing line, and thus separate to our perception the living from the dead parts, of which, in various proportions, all organised beings are made up.

CHAPTER IV.

THE ANATOMICAL SEAT OF THE LIVING MATTER.

§ 78. We now come to one of the most remarkable anticipations, by deductive reasoning, of a great discovery, subsequently made by others, which the history of science can display. We have repeatedly said that the proximate principles do not exist in the living state, but only come into existence on the rearrangement of particles which takes place at death. But where is that death, and in which part anatomically does that change take place at the moment of general death of the individual? Are we to imagine that an organised being—say a man weighing 150 lbs.—is composed of a mass of matter in the unknown metabolic state of affinity in which it differs chemically entirely from what it shall be within a few minutes, or at most, hours, after death? This is not by any means Fletcher's meaning; on the contrary, he holds that during life the greater part of the framework of our bodies is in the same condition, chemically and physically, as we find it after death. In the first place, although all organised beings consist of both fluids and solids, he denies vitality to the fluids properly so called *per se*, going very fully into the question of the vitality of the blood and deciding against it. No objection will be made to such a decision now as respects the fluid part of the blood, and since the discovery of the living nature of the white corpuscles some of the apparent arguments for the vitality of the blood as a whole are explained and the possession of that property by the solids alone vindicated. The whole fluids (and all crystallisable substances) being thus subtracted from the living matter, a large part of the ternary and quaternary chemical principles are thus admitted to exist in the living body. Of the solids there are some tissues and parts, such as hair, epidermis, horn, nails, &c., which are admitted by all to be the chemical compounds we see. There remain, then, the solids composing the proper tissues and parts which, as a whole, take part in vital functions. Then he says, "Admitting that irritability or vitality, general and specific, is a property of the organised solids alone, it becomes a question of the highest interest whether it be directly inherent in each of the organised tissues, either of plants or animals, or whether it merely appears to be possessed by them all in virtue of some one which is universally distributed over the organised being and inextricably interwoven with every other" (ii, p. 55). This question he examines particularly in respect to the two which display vital properties in the most palpable manner, viz. the muscular and nervous tissues. It must be remembered that with Fletcher irritability is synonymous with vitality, and is the property on which, with concurrence of stimuli, conditions and pabulum, all vital actions depend, including development and renewal of itself as well as all other tissues, and the fluids that are not possessed of this property. His reasoning is based chiefly on arguments from comparative anatomy and physiology, and he notices that, although quite the lowest tribes of animals and all plants are destitute of a distinct nervous and muscular system, yet they distinctly show the possession of irritability, not only by movements but the whole range of the essential vital functions, such as growth and development, and that, therefore, the irritable matter exists in them in a diffused form, and that all possess, in a rudimentary form, a certain degree of the faculties which only become perceptible in the higher animals through development and specialisation. That "the splendid human organism itself consists merely of the same organs, regarded fundamentally, as exist in the polype, the differences consisting in their different degrees of elaboration" (p. 37). While, again, "the tendency to concentration or centralisation in this respect is, in general, in the direct ratio in each of the advancement of its organism, or its rank in the scale of organised beings"* (p. 19). By an interesting train of reasoning he comes to the conclusion that irritability (vitality) does not reside in the muscles, nor in the nervous system as a whole, but exclusively in the grey matter of the ganglionic nerves alone, and that it is only in virtue of the possession of this matter universally diffused and intimately interwoven with its texture that any tissue or part possesses vitality. Hence, he "must deny any direct participation in irritability or vitality" to "those peculiar aggregations of matter which go to form respectively the cellular, dermoid, mucous, serous, fibrous, vascular, osseous, cartilaginous, or muscular tissues" (ii 92). The grey matter of the spinal cord and brain have likewise vitality and the superadded, but localised, faculties of sensibility and of thinking respectively. Thus, with the exception of the grey matter of the brain, of the spinal marrow, and of the ganglionic nerves, the whole structure of the highest organised beings is actually composed of dead and now merely chemically combined matters. Here, then, is ample scope for the display of those mechanical and chemical actions which are certainly largely represented in the functions of living beings, without trenching on the truly vital actions which a large school are inclined to thrust away altogether. We may, without difficulty, now perceive how the bones give firmness and support; how the teeth grind the hardest substances; how the fibrous, elastic, and connective tissues perform their respective physical functions; how the muscles form an apparatus admirably adapted for the physical conditions of motion in particular directions, while probably both a chemical and a vital process are involved in the transformation of force; how osmosis, chemical fermentation, interchange of oxygen and carbonic acid by the hæmoglobin, and all the various processes, strictly physical and chemical, of animals and plants are performed in harmony with vital action. Anatomically it may be said that this doctrine is erroneous as applied to the ganglionic nervous system, as it is usually described, but it really applies to a hypothetically supposed, universally diffused, grey matter, which was, of course, not seen by him or any one else then, nor were the means existing of discovering such matter at that time. His view of the actual ganglionic nervous system, as presented by systematic anatomy, was much in harmony with the most modern conception, in particular that the fibres consisted mainly of filaments derived from the cerebro-spinal system, and especially an extension of Charles Bell's respiratory system of nerves, which, according to Fletcher, constituted a complete system of nerves of organic sympathy corresponding to the reflex action now usually spoken of. Fletcher's respiratory system has not yet received the attention it merits from physiologists, but it cannot be entered upon here. As the grey matter alone is with him the sole seat of vital properties, and these, of course, are inherent and incommunicable, the diffused ganglionic matter is that which is of importance, and the fibrous part of the ganglionic system probably belongs entirely to centrifugal and centripetal fibres of communication for the conveyance of stimulus. From the restriction of all vital properties to the grey matter alone it follows that all fibres of nerves, of whatever denomination, must both begin and end in grey matter, either the diffused ganglionic nervous matter of the tissues, or the grey matter of the brain and spinal marrow; this also will be found in harmony with Beale's discoveries of the terminations of nerves which are still contested by the German school.

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* Mr. Darwin (*Descent of Man*, p. 211) attributes to Von Baer "the best definition ever given of advancement or progress in the organic scale; and this rests on the amount of differentiation of the several parts of the same being, when arrived, I should be inclined to add, at maturity." Nothing can better show the unmerited oblivion into which Fletcher's *Physiology* seems to have fallen, than the circumstance that Mr. Darwin is apparently not aware of its existence. Nevertheless, those chapters on concentration as an evidence of advancement contain matters of great interest on his subject, and they attracted attention at the time when the book called *Vestiges of Creation* was in vogue.

The extreme precision of Fletcher's ideas in separating irritability from sensibility and the conveyance of any communicable influence whatever distinguish his view of the ganglionic nerves from that which represents them as nerves of sympathy, as their original name implies, and also as a whole the nerves of organic life, such functions really implying both susceptibility of perceiving and the power of generating and of conveying stimuli; in fact, the process of reflex action, which can only be performed by a complicated apparatus. It is, therefore, in relation to the admixture of fibres of different portions of the nervous system that the ganglionic system, as it appears in non-microscopic anatomy, is to be judged, and, if so, its size corresponds to its function, which would not be the case if all we saw were the sole seat of vitality for the whole body. We must also notice what Fletcher gives as one of the proofs of its function as the seat of vitality, viz. its early appearance and great predominance of size in the fœtus and also in the young over the same animal as life advances, exactly as we would expect of a substance through which formation of tissues and organs as well as all other vital functions is performed. Whatever we may think, in respect to systematic anatomy, of Fletcher's theory of the functions of the "great sympathetic," there is no doubt that his hypothesis of the physiological nature of the ganglionic nervous matter is very remarkable and well worthy of attention. It amounts simply to this, that living beings are living beings solely in virtue of their possession of a "pulpy, translucent, homogeneous matter, yielding, after death, fibrin" (p. 106), which is so universally distributed and interwoven with all organised tissues as to form a considerable part of their bulk.

§ 79. We have here a physical basis for vital action, for it was scarcely conceivable that anything exhibiting structure or firmness of texture beyond that of a mere pulp could possess the requisite degree of molecular mobility for the display of the rapid and almost continual changes which go on in a substance whose molecular actions are so complex. But although the irritable matter possesses certain characters in common in all parts, not only of the same animal, but in all animals and plants, thus manifesting a generic unity, yet, as before said, there are differences in the properties of each animal and plant, and each part of all individuals, implying a corresponding difference in the composition of the irritable matter—facts embodied in the doctrine of "specific irritability," which asserts that "every organised solid is not only the seat of irritability in general, but has a kind of irritability peculiar to itself" (p. 52); which specificity applies to all its faculties, viz. the plastic, metabolic, and germinative. Further, all changes of the vital properties must be rigidly traced to corresponding changes in the quantity and composition of the irritable matter.

> "From what has been above said of the nature of irritability and irritation—or irritability called into action by the requisite stimuli—it will readily appear that the practice of speaking of irritability or vitality as something substantial, something which may be *per se* accumulated, diminished, exhausted, is erroneous; but the error amounts merely to this, that, for shortness sake, we speak of the *property* as directly so affected, whereas it is, in fact, the *substance* of which this property is the attribute that is so, the property, however, manifesting, of course, corresponding changes, since the degree of irritability in any part will necessarily be in the direct ratio of the quantity and quality of the irritable matter which it contains" (p. 125, ii *b*).

Every vital action presumes a metabolic change in the irritable matter, and when that change results in the formation of tissue or secretion there must be a transformation of matter from the metabolic to the chemical state, and thus consumption of the irritable, living, or diffused ganglionic nervous matter must take place. I may add that the same, with satisfaction of stronger affinities, must take place when any extrication of force takes place, either for work or heat or the internal stimuli. At the same time, as the living matter is consumed, it is continually renewed from the blood, or other nutrient fluid, in the same proportion as it is consumed, or in greater proportion, if growth of living matter is going on. When these processes are exactly balanced health is the result, but any excess, deficiency, or perversion of the pabulum or stimuli disturbs the balance, and disease, and beyond certain limits death are produced. These changes take place in the irritable matter itself, independently of a vascular system, as respects both the stimuli and the pabulum, but the action of the stimuli is more perceptible in the higher animals, where they can control the supply of pabulum; accordingly, Fletcher states it as a principle that the formation of the living matter itself, and all secretion and nutrition depending on it, are, *cæteris paribus,* in the direct ratio of the quantity of blood in the capillary vessels. These last, therefore, play the most important part in changes of nutrition and secretion and in inflammation, although still all vital action in them is exclusively seated in the living matter interwoven with the tissue proper. Quantitative variation, either by excess of production or by exhaustion from over-consumption of the living matter, has thus for its chief factor the abundance of the supply of nutrient matter.

Mere change of quantity, however, is not the only variation that can take place in the irritable matter, but also change of quality from the action of excessive or abnormal stimuli or insufficient or abnormal pabulum. In this case disease and death are said by Fletcher to result from deterioration of the ganglionic nervous matter in any tissue or part when local, or the whole body when general. This *deterioration* of the irritable matter from excessive stimuli plays an important part in Fletcher's system when he comes to pathology, for it applies not only to the metabolic faculty in altering secretion and nutrition, and thus giving rise to a variety of morbid products all of which display the character of lowered vitality; but also to the germinal faculty in producing living matter of a degenerated character.

This last is very important, and rests on the principles insisted on frequently, that secretion, nutrition, and germination are all intimately connected as varieties of vital action, that new formations and growths are not deposited from the blood, but consist in "a proper germ or matter secreted by the vessels, and containing within itself the rudiments of its own organism" (p. 142). Again, "the organisation of the embryo was the work of its parent, its organogenesy is its own, and parts which it received from the former in a state of ambiguity and diffusion itself renders concentrated and determinate." (II, p. 24.) That the germinal faculty is not confined to the reproductive organs of the individual, has been stated at the note to § 66, p. 120. The dependence of the germinal faculty on stimuli, just as all the other vital faculties, is, likewise, enforced by sufficient reasons, and all spontaneity or autonomy of the living matter denied in this any more than other instances. In short, every function of living matter is reduced to dependence on adequate causes, although these are as yet mostly quite unknown, exactly in the same way as the phenomena of the inorganic world. Fletcher does not hesitate to uphold and push to its legitimate conclusion the Brunonian doctrine, that all positive agents act as stimuli on the living matter, and are essential to the actions we call life. There are no direct sedatives, and all deleterious agents, even the most rapidly destructive, such as excessive heat, electricity, hydrocyanic acid, and serpent poisons, kill by "deterioration" of the living matter to such a degree that renewal from the pabulum is impossible, or else depress temporarily by excessive constriction of the capillaries, shutting off the supply of pabulum for a time. For this latter reason the primary excitant action of all stimuli is generally overlooked, and in fact forms, according to Fletcher, the latent stage of acute disease and the action of poisons, because, owing to the capillary constriction, the increased formation of irritable matter that would take place cannot do so owing to the deficient supply of blood. Consequently till that is allowed by the relaxation of the capillaries (or what corresponds to that, the relief of the spasm, allowing the return of the peristaltic action), the effect of stimuli in morbid increase of growth of living matter, or of its deterioration in altered quality, cannot be manifested. The so-called physiological or pathogenetic effects of poisons and medicines, and of the exciting causes of disease, belong, therefore, to the *second stage* of their operation, and that stage is always one of lowered vital action, or both lowered and deteriorated action. Hence the important therapeutic principle, that all remedies acting on the diseased part act as stimuli to it, either indirectly, as counter-irritants through the nerves of organic sympathy, or directly, as specific stimuli adapted by their specific properties to act on the specific irritability of the part or the specific character of the deterioration. The subordination of the germinal faculty likewise to the action of stimuli is of great moment for the hope of therapeutics, as it is only, according to Fletcher, by "a regeneration of the irritable matter" (p. 145) that recovery can take place. The principle of deterioration of the living matter by morbid causes acting on the germinal faculty in particular throws light also on the operation of the remote causes of disease.

In accordance with these views, I wrote a paper in April, 1868, explaining the nature of dormant disease, by referring it to a gradual change in the composition of the living matter induced by the slow operation of abnormal stimuli or pabulum of a specific character. This change, not being sufficient to cause quantitative disturbances in the living matter, may go on for long without any marked symptoms, till at length the supervention of any of the occasional non-specific exciting causes of disease, such as cold, injuries, digestive disorders, &c., produce their usual acute and quantitative disturbance, and this combined with the previous alteration of the quality of the living matter gives the disease its specific character—such as gout for example. I proposed, therefore, to define dormant disease as that in which the qualitative is dissociated from the quantitative element, and considered that it is always of a lowered or degenerated character in comparison with health.

These few remarks on the bearing of our subject on pathology and therapeutics may serve to show the harmony between Fletcher's views and those of late becoming generally prevalent. This could be done also to a much greater extent if necessary, but probably the above will suffice, and we shall now conclude this part of the subject by stating the singular dilemma into which Fletcher now falls.

§ 80. He speaks constantly of the vital functions of the several tissues, while, at the same time, life is with him merely a series of actions depending on the properties of the substance in which they take place, and, therefore, which cannot be communicated or conveyed in any degree. With him, as regards the higher animals, the parenchyma or capillary tissue was the "seat of all the molecular actions of the body;" thus including secretion and nutrition not only of all other tissues but of the ganglionic nervous matter itself. "But the ganglionic nervous tissue, assuming this as the immediate seat of the property in question, is, of course, continually renewed and consumed, like all the other tissues of the body, by molecular processes, the seat of which is the parenchyma; which thus effects incessant changes in this tissue, not only as interwoven with the substance of every other organ, but also as entering into the composition of the minute vessels of which itself consists" (p. 125).

How can the parenchyma or the capillary walls take any vital part in the formation of the substance, whose presence alone constitutes their vitality, and yet which property cannot be transferred to the smallest distance? Speaking in the present language of the force-doctrine, the metabolic power is not the offspring of a force, and therefore cannot be conveyed or transferred through other kinds of matter or the interstellar æther so as to operate at a distance, but it is the result of an affinity proper to one particular aggregation of matter alone, and can, therefore, only act upon particles within that, nor can any catalytic action take place between portions of matter in the metabolic and the chemical states of combination respectively. The difficulty was not overlooked by Fletcher, who says (p. 92, II):

> "With respect to the manner in which irritability or vitality—presuming that its immediate seat is, at least in all the higher tribes of organized beings, the ganglionic nervous tisssue—may be supposed to be extended to the other organized tissues, so that each appears to be *per se* possessed of it, very little needs be said. In the case of sensibility, the immediate seat of which is the sensiferous nervous tissue, there is no difficulty in at once admitting that this is the exclusive seat of the property in question, and that the comparatively few other tissues which appear to be *per se* possessed of it, are, except in as far as they contain this, entirely insensible. But with respect to irritability, we may have more hesitation in confining this property to the ganglionic nervous tissue, since irritability is unlike sensibility, not merely an adventitious, but an essential property of organized beings; and it seems, therefore, to be necessary not only to every organ and every organized tissue, but also to every point of such tissue, in order to effect its molecular actions. But we must remember that there is no point of any organized tissue which does not contain ganglionic nervous matter, such matter being essential to its organism; and consequently there is no point which is not possessed of the property in question, which is accordingly, as has been from the first insisted on, the result of such organism. It is true, unless every organized tissue be one uniform mass of ganglionic nervous matter, there must be interstices of some other matter. Thus there must be spaces in the ganglionic nervous tissue occupied by one or other of those peculiar aggregations of matter which go to form respectively the cellular, dermoid, mucous, serous, fibrous, vascular, osseous, cartilaginous, or muscular tissues, wherever an organized part presents itself. But the spaces thus occupied are almost infinitely minute, and such as to be perceived only by the mind's eye; and we may, therefore, without doing much violence to preconceived opinions, deny that the tissues occupying them have any direct participation in irritability or vitality. As merely a property, it can never be directly transferred; and if it be immediately it must be exclusively that of the ganglionic nervous system. But such interposed substances, thus destitute *per se* of irritability or vitality, are not, nevertheless, in the condition of merely inorganized matter—of a mere 'solidum

mortuum,' with respect to either their structure, their composition, or their actions. It is never otherwise than as combined into a whole with ganglionic nervous matter that they actually exist; and although the former alone perhaps has a structure and composition which can be strictly called vital, and is alone susceptible of vital changes, still the latter are not less *sui generis* in these respects, possessing as they must do such physical and chemical characters as qualify them to co-operate in these changes, and thus to temper and modify in every part the resulting phenomena. It is easy to conceive that, if a flame be applied to a mixture of combustible and incombustible matters, the former alone will burn, but the latter may still be competent to undergo, each according to its peculiar character, such modifications of expansion, liquefaction, vaporisation, incandescence, and so forth, as will materially qualify the nature of the combustion, and may even be essential to it. It is in a similar relation that the substances occupying the interstices of the ganglionic nervous matter, in the several organized tissues, appear to stand to the nervous matter itself—they have *per se* no irritability or vitality, nor are the phenomena which they display primarily vital; but they are, nevertheless, such as no merely inorganized matters could have presented, and such as not only temper and modify vital action, but are perhaps necessary to render such action available."

It cannot be said that this is satisfactory, nor is the doctrine put forward as otherwise than a hypothesis resting upon presumptive evidence alone. That evidence was, however, so strong that a man of powerful intellectual grasp like Fletcher did not hesitate to accept the conclusion to which it pointed, in spite of the apparent inconsistency involved therein, in the persuasion that time and the progress of discovery would realize what was true in the previsions of deductive reasoning by giving us new facts to fill up the blank and explain the apparent inconsistency.

The pupils and disciples of Fletcher have waited well nigh thirty years in the hope that such a time would come, although, perhaps, not in their day. But the time has come, and with it the explanation, which in some respects is so simple, and so completely in accordance with Fletcher's whole theory of the living state, that it strikes us as strange that we did not see it sooner, and now we can hardly state Fletcher's position without falling into the language of the new theory of growth and nutrition of tissue.

§ 81. The death of Fletcher seemed to occur at the close of a period in the history of physiology, for he was one of the last writers of a consistent system of physiology before the recent great advance in microscopic histology took place, and before the discovery of the conservation of force doctrine introduced a most important element into all physiological investigations. For about ten years after his death there was a dreary period, in which the unfortunate revival of humoralist theories by the Vienna school of pathology occupied men's minds, and was most unfavorable for the fair hearing of a purely solidist system. It is well said by Virchow (*Cellular Pathology*, p. 2) that every important epoch in medicine has been ushered in by important discoveries concerning the structure of the body. With Fletcher, we have seen, the capillary or parenchymatous tissue was the last word of anatomy; beyond that we could not go for a material seat of the true vital actions of growth and nutrition. But even while he wrote Schleiden had begun to make those observations which were afterwards followed up and developed by Schwann into what is known as the cellular theory. It is not my intention to go into the history of this large subject farther than to allude to the cardinal point in which it touches our subject, which is that a complete revulsion from humoralism again took place. Besides that, the doctrine of a central overruling vital principle received a shock, and that of the inherent and special vitality of certain anatomical elements in all tissues and parts became recognised. This is, in fact, nothing but the specific irritability of Fletcher, and now his other doctrines, one would have hoped, might have a prospect of a fuller hearing; but the time was not fully come yet. For by Schwann and his followers the cell as a whole, and more particularly the cell-wall, was looked upon as taking an active part, or being, in fact, the seat of the vital process and former of tissues.* If this be allowed we are not advanced beyond the stage where Fletcher left us, for the cell wall is, in many cases, of firm texture and chemically different from the soft cell contents. It likewise passes insensibly often into the intercellular substance of which the tissue is said to consist. It is, therefore, open to all the difficulties enumerated at § 80 against the vital or metabolic action at a distance supposed to be exerted by anything, and more especially by a rigid structure. It likewise is incompatible with the hypothesis of vitality being seated in "one homogeneous, pulpy matter, yielding fibrin," for it differs in every conceivable way in kind, texture, and composition in all animals and plants, and in different parts of the same organised being. However, even before the cellular theory was complete its foundations were sapped, and now we may already look on it as a theory of the past, at least in a physiological sense, for the cell still takes its rank as the anatomical elementary part, and represents to a great extent the substance and function given by the older anatomy to the capillaries.

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* "So far from being the centre of activity of the vital actions, it [the endoplast—central portion] would appear much rather to be the *less important histological element.* The *periplast,* on the other hand, which has hitherto passed under the name of cell wall, contents, and intercellular substance, is the subject of all the most important metamorphic processes in the animal and plant. By its

In 1835 a living moving substance was discovered by Dujardin in the lower animals, which he called *sarcode*, but for long this was looked on as something different from the cell. Afterwards a similar substance was found by Von Mohl in vegetable cells, and named *protoplasma*. The attention of a long list of eminent physiologists has been directed towards these substances, and finally, chiefly through Cohn, Leydig, Max Schultze, Kühne, Brücke, Häckel, and others, they were shown to be of the same nature, and gradually they have assumed a position of supreme importance, and, in fact, seem to take the place of the cell altogether. With the German school that is not allowed as yet, and they still cling to the idea of the cell, although they admit the existence of naked masses of protoplasm,

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differentiation every variety of tissue is produced; and this differentiation is the result, not of any metabolic action of the endoplast, which has frequently disappeared before the metamorphosis begins, but the intimate molecular changes in its substance which take place under the guidance of the 'vis essentialis.'" (Huxley, *Brit. and Foreign Rev.*, Oct., 1853.) This may be taken as a correct statement of the opinions then prevailing, but they are quite erroneous, and are abandoned by Huxley himself now.

apparently having separate existence and vital powers formerly attributed to cells.

§ 82. From what is said in the last paragraph it is evident that the dawn of the day was approaching which might realise the hypothesis set forth in the preceding pages. And so it has proved, for in 1861 Dr. Lionel S. Beale delivered before the Royal College of Physicians, and afterwards published, in the same year, in his *Archives*, a course of lectures "On the Structure of the Simple Tissues of the Human Body," wherein was proclaimed a discovery which is destined to have an influence on physiology which cannot yet be appreciated, viz. that of the true anatomical nature of the living matter, and the sharp line which divides the living from the dead in all organised beings. Such a discovery is surely, in intellectual merit, equal to, if not greater than, that of the circulation of the blood, although its effect on the progress of science cannot be expected to be comparable to that, for the times are very different and no one can now found anew the science of experimental physiology, and hardly even a new method in its special departments.

In a field where there must be so many labourers, as in physiology at this time, the steps which lead up to the discovery of any great principle must be the work of many hands, but the claim of Dr. Beale, as the first who perceived the unity and full significance of the protoplasm, can hardly, I think, be disputed, and his name will stand forward in future times as the pre-eminent one, while most of those who are now mentioned as on a level with, or even above him, have faded into oblivion. In the discovery of the chief facts on which the doctrine rests, priority must also be given to Beale, for it was only after years of patient research and experiment—during which he discovered new methods of histological analysis, enabling him to surpass the microscopic preparations of others as far as Ruysch did the injections of his contemporaries—that he accumulated sufficient evidence for the establishment of the doctrine. He says in the preface to the first publication of the lectures in 1861, "he thinks it right to state that the conclusions which have now assumed a definite form have gradually grown upon him during the course of observations extending over a period of several years. In fact, some of the drawings in this volume, and others which have been published elsewhere, equally favorable to this view, were made long before any specific theory had been arrived at."

All that has been done since can merely confirm or refute the doctrine, but can have no share in its discovery. It is therefore a matter of satisfaction that the man of genius to whom it is due should be one of our countrymen, and that he has again gained for England the palm of physiological discovery. The German school (in the race with whom we are as yet so heavily weighted from the want of state aid to physiology) are, in truth, hardly prepared to dispute it, as they do not accept the doctrine in its full significance and are still occupied with contests over the details of the cell theory, * and are also for the most part too much

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* In 1861 Max Schultze wrote a paper in *Reichert und Dubois' Archiv*, in which he showed that the cellular theory, as then held, required great modifications, and that the cell contents, especially the parts corresponding to Von Mohl's protoplasm, was of higher importance, not only for the life of the cell itself, but for the building up of tissues. In his work *Das Protoplasma der Rhizopoden und der Pflanzenzellen*, 1863, he recapitulates some of the positions of the above paper, and enforces the necessity of those modifications by saying that "the necessity for such improvements is also felt in those quarters where the cellular theory has not yet been understood; this applies to the treatise of Beale (*Lectures* of 1861), which will not receive the attention which it, in many respects, deserves, because it stands outside of the cellular theory. Beale's germinal matter, certainly, is essentially that which we call protoplasm comprehending the nucleus, and the formed material is brought into what is essentially a true dependence on the protoplasm. But of cells as elementary parts or elementary organisms, and of nuclei as different from the protoplasm, and still so necessary to it, there is no word. For him (Beale) the great and inalienable [unveräusserliche] discovery of the cell is of no more than historical interest" (p. 3). Max Schultze then goes into the details of the subject, and in the great majority of points comes to conclusions almost identical with Beale's. In these, also, Brücke and Häckel, for the most part, agree, while Reichert takes the opposite side, and defends the older views of the cell. Schultze, however, never gives up the importance and the vitality of other parts of the cell when present, and concludes with the asseveration, "I must again expressly declare that Reichert's fears are completely groundless, that by my conception of the cell the bases (Grundvesten) of the cell theory are shaken. No one can be more deeply penetrated than I am with the conviction that the doctrine of

trammelled with chemicalist hypotheses to take much interest in the fixing of a sharp boundary line between the living and the dead. But the time will ere long come when pure vitalism will regain its ascendancy, and the full significance of the principle will be generally recognised. At present the vitalist school even are hardly prepared to accept it, and many of those who receive as true Beale's doctrine of nutrition and secretion in a general way cannot bring themselves to believe that the greater part in bulk of all tissues, even the muscular and nervous, hitherto deemed *par excellence* the very seat of life, are actually dead, as dead as hairs, nails, and cuticle, all our lives long.

§ 82*a*. The word protoplasm has of late got rather into disrepute through Mr. Huxley's popular lecture on the "Physical Basis of Life" at Edinburgh, reported in the *Fortnightly Review*, vol. v (1869). The doctrines there put forth need not have excited displeasure, for, with the exception of a certain levity in the mode of statement, they are substantially the same as those taught by Fletcher up to 1836. The question put by Huxley, "Is the matter of life composed of ordinary matter, differing from it only in the manner in which its atoms are aggregated?" (p. 136), is somewhat more precisely stated and answered in the affirmative by Fletcher. But Huxley does not see so strongly the immense distinction between the vital and chemical state of aggregation, and thus falls into the inconsistency of speaking of "ready made" protoplasm, and even protoplasm of such a proverbially dead substance as mutton, being "dissolved" and "transubstantiated" into his own. He speaks likewise of protoplasm as an albumenoid. With such chemical views he is obliged, like the rest of the mere chemicalist school, to fall back upon "subtle influences" to aid mere solution in converting mutton into living matter. In fact, his

the cell, as the fundamental element of all animal tissues, is, for all time, inalienably assured" (p. 63). This, be it observed, is written in 1863, and when commenting on Beale's lectures. Now, Max Schultze and Brücke are, of all observers, the nearest to Beale in their views of protoplasm; and if they repudiate both Beale's facts and his theory, it is obvious that henceforward no one else can dispute with Beale the claim to the discovery, if it be one, that the sole living part of all organized beings at all stages of their life, from the polype to man and from the fungus to the oak, is that "clear, structureless, formless stuff" now called protoplasm. I claim no share in the actual discovery for Fletcher, but I think it is a high merit to have demonstrated, a quarter of a century before, that some such substance was to be discovered; and I hope it will serve to attract towards his complete system of physiology, pathology, and therapeutics, the attention it deserves from professed physiologists.

theory is to a certain extent a reversion to the hypothesis of organic molecules which were supposed to build up the different organised beings by their perpetual transmigrations from one to another through the aliment. This hypothesis was judged and pronounced untenable by Fletcher (p. 154). If the author of the tract *As Regards Protoplasm* wishes properly to criticise the doctrine of a Physical Basis of Life, he should go to the fountain-head and read Fletcher's *Rudiments of Physiology.* In the meantime, however, Dr. Beale, who was then giving his Oxford lectures, pauses in the middle, and takes Huxley to task for his strange views in applying the term protoplasm to dead lobster, mutton, bathybius, albumen, and other things. The word protoplasm was invented by Von Mohl to designate a certain part of the constituents of vegetable cells, and when adopted by others it was applied by them to a great variety of substances, in accordance very much with their theories of correspondence in vital function with the original matter. The consequence has been that it has come to mean with different authors, in addition to its original signification, the cell-wall, other cell contents, the whole cell, mucus, various granular and fibrillar structures, the contractile matter *par excellence*, intercellular substance, nerve-fibre, nerve-cell-walls, bathybius, roast mutton, bread, and many more things. For these reasons Beale objects to its retention as the designation of the living matter, and since for several reasons his own original term germinal matter is inconvenient and imperfect, as applying to one faculty of the living matter alone, he proposes a new name instead of restricting the word protoplasm to one accurately defined meaning:

"The name I propose to give to the living or germinal self-increasing matter of living beings, and to restrict to this, is Bioplasm. Now that the word Biology has come into common use, it seems desirable to employ the same root in designating the matter which it is the main purpose of biology to investigate. Bioplasm involves no theory as regards the nature or the origin of the matter; it simply distinguishes it as living. A living white blood-corpuscle is a mass of bioplasm, or it might be termed bioplast. A very minute particle is a bioplast, and we may speak of living matter as bioplasmic substances." (*Microscopic Quarterly*, July, 1870.)

If the name bioplasm had been given when the thing was discovered probably no one would have objected to it, but it is different now; and when Beale's germinal matter is, in the great majority of cases, exactly the same object which has long been described as protoplasm by many naturalists, the scientific world may be more willing to accept from him a more precise restriction of the term than a new one altogether. We cannot tell yet how this will be, and although Dr. Beale's wish will have, of course, more weight than that of any single individual, it may not prevail. There are many inconveniences attending the continual introduction of new names. Of this Fletcher was particularly careful and refused to invent any new name, although he was far from being satisfied with irritability. That word had been used in a great variety of senses since the time of its inventor, Glisson, quite as many and as little consistent as protoplasm, and Fletcher's meaning corresponded exactly with none of them, yet he refused to invent another word, but proposed simply to define it more strictly, for these reasons:

"There seems to be no advantage, however, but, on the contrary, very great inconvenience in the endless invention of new terms to which modern philosophers have been in general so addicted, and by which many who are incapable of distinguishing themselves in any other way have sought distinction. Many better and less equivocal terms than irritability might unquestionably have been originally invented to express the faculty under consideration; but no man has a right in the present day to expect that *his* new nomenclature, however abstractedly excellent, will entirely supersede the employment of a term already in general use; and if this be not the case, the onomatomaniac, without obviating the necessity of our understanding old terms, only imposes upon us that of learning new ones likewise. Names in general are—

"'Not bad *simpliciter*, nor good,
But only as they're understood,'

and it seems quite sufficient to establish any given name as the best, that it is generally employed, and by all who employ it once clearly defined." (p. 51.)

Fletcher was probably alluding to Mason Good, and such like literary interlopers in physiology, but the common-sense rules to which he submitted himself must be generally applicable. Besides, it does not appear that bioplasm is abstractedly more suitable, for it may mean "that which is formed by or through life," an appellation that would apply equally to the dead "formed material" as to living matter. According to Richerand, "Anatomy is the science of organism," while "Physiology is the science of life;" so for a name describing the anatomical seat of living matter protoplasm would be better, as it means "that which is first formed;" and as it is impossible to give a complete epitome of attributes of a thing in its name, it would be better to embody the cardinal point in Beale's own theory, viz. that no pabulum can take part in living action till first converted into this substance. If the word life is to be brought into the name it might be bioprotoplasm, which would be a clumsy word.

An excellent classical scholar, Mr. Scott, of Birmingham, to whom I submitted the question, gives this opinion:—"Bioplasm is very vague. If I heard it for the first time without knowing anything of the connection in which it occurs I should take it to mean an image of life. It might be used to mean 'anything formed or moulded by life;' but there is nothing *in the word itself* to indicate that the thing formed is also itself living."

On the whole, it is most probable that the term protoplasm will still be retained in this country to express Beale's germinal matter, as it is so accepted by Dr. Sharpey, whose calm and solid judgment reveals to us, as it were, the verdict of posterity. I will therefore continue to consider as synonymous terms "living matter," "irritable matter," "diffused ganglionic nervous matter," "germinal matter," "bioplasm," and "protoplasm," but will use chiefly the last and "living matter."

§ 83. As the results of those years of independent observation, and resting on them alone, Beale came to the conclusion that life or vital power is not generally diffused over different tissues or organs, but is restricted to one kind of matter alone in all living things, both animal and vegetable, from the highest to the lowest. It is in this substance, which he terms *germinal matter,* alone that "take place all the changes which more especially distinguish living structures from lifeless matter. The particles of which this is composed, after passing through certain definite stages of existence, undergo conversion into the peculiar substance or substances they were destined to produce. It is the germinal matter alone which is capable of *forming,* producing, and converting. The matter external to it (cell-wall, intercellular substance, or fluid) has been *formed* or produced, and it may be changed, but it has no power to *produce* structure or to alter itself." (*Arch.,* III, p. 116.)

By the use of these terms, "germinal matter and formed material," he designates the two radically and irreconcilably distinct states of matter of which in various proportions all living things are composed. By means of this clear distinction he brings order into the chaos still existing in the cell theory, and shows that the sole active matter at one time corresponds with the nucleus, at another to both nucleus and cell contents, again to the matter lying between the cell-wall and certain of the cell contents; while, again, the formed material in some cases corresponds exactly to the cell-wall; in others, to cell-wall and part of the cell contents; in others, to the intercellular substance, and all the tissues and fluid secretions, and, in fact, to every part of living beings formed by vital action. To us the essential point is, not to reconcile the various theories of the cell structures and contents; but what is the anatomical nature of this one true and only living matter?

"Germinal or living matter is always transparent, colourless, and, as far as can be ascertained by examination with the highest powers, perfectly structureless, and it exhibits these same characters at every period of its existence* (36). However much organisms and tissues in their fully formed state may vary as regards the character, properties, and composition of the formed material, all were first in the condition of clear, transparent, structureless, formless, living matter" (p. 46).

"There is a period in the development of every tissue and every living thing when there are actually no structural peculiarities whatever; when the whole organism consists of transparent, structureless, semi-fluid, living germinal matter; when it would not be possible to distinguish the growing, moving matter which was to evolve the oak from that which was the germ of a vertebrate animal. Nor can any difference be discerned between the germinal matter of the lowest, simplest, epithelial scale of man's organism, and that from which the nerve-cells of his brain are to be evolved." (*Oxford Lectures*, p. 608.)

Beale's germinal matter is identical with the protoplasm of the German school, as found in the amœbæ, white corpuscles, pus-corpuscles, &c.; so the description may be supplemented by the statement that it coagulates between 35° and 50° Cent., contracts under electricity, stiffens after death, and is then found to contain myosin and other nitrogenous proximate principles.

Here, then, we have the exact counterpart of Fletcher's irritable or diffused ganglionic nervous matter brought forward as the result of independent research by pure observation and experiment. This is surely a very remarkable fulfilment of the previsions of deductive reasoning!

It is quite unnecessary to go into the full consideration of Beale's wonderful discovery, as his theory of nutrition must be known to every one, so I will merely touch upon a few of the points which show correspondence with Fletcher's theory or clear up its difficulties.

With respect to the comparative bulk of the protoplasm and the lifeless tissues, there is no fear that we cannot account for the apparent inherent vitality of the smallest parts accessible to our senses, for Beale says, "There is not one portion of a living thing much more than $\frac{1}{500}$th of an inch in diameter in which living matter cannot be demonstrated." (*Medical Times*, April, 1866.)

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* Beale, *Protoplasm*, 1870.

With respect to the specific irritability inherent in each portion of living matter—

"For the characters and composition of the living matter do not enable us to premise anything whatever concerning its formative properties. In the formation of man and the higher vertebrata the primary mass of bioplasm or living matter absorbs nutriment and grows, and then divides and subdivides into numerous masses, which are arranged in a definite manner, but what determines this is not known. From each of these in preordained order, and with perfect regularity more are produced, no doubt, according to 'laws,' but laws which we know nothing about, except that they are not physical. As this process of division goes on the resulting masses produce various substances, some having wonderful structure and properties. But the power of each series to produce these peculiar materials which did not exist before, and which cannot be extracted from the food supplied, differs from that of the series which preceded it, and so on, until the complex structural basis of the organism is, as it were, laid down. There are masses of bioplasm to form nerve, others to produce muscle, others glands, and so on, all of which have been derived from one common mass; but the bioplasm destined to take part in the development of a gland will, under no circumstances, produce muscle or nerve. And yet with all this marvellous difference in power, which seems to be somehow acquired as development advances, there is, as far as is known, no difference in matter. The nerve- or muscle-producing bioplasm is, as far as can be ascertained, the exact counterpart of the gland- or bone-forming bioplasm, and why one produces one tissue and the other a very different tissue cannot be explained."

Germinal matter of bone transplanted.—"When periosteum is transplanted, as was done by M. Ollier, of Lyons, to various parts of the organism, the production of the bony tissue formed in the new situation is due to the growth and development of the masses of germinal matter which exist in such great number at the deep surface of this fibrous membrane. These grow and multiply, and produce formed material, just as if they had remained in the original seat of their development—a striking proof that the *kind* of tissue formed by germinal matter depends upon its *powers* rather than position or the conditions to which it is exposed. In certain forms of bone cancer very minute portions of actively growing germinal matter are sometimes carried to the lungs, and grow and multiply, and give rise to bone cancer in the pulmonary tissue, showing that the germinal matter possesses the peculiar property or power of giving rise to this particular tissue if supplied with pabulum." (*Oxford Lectures*, p. 112.)

He illustrates the consumption of protoplasm and constant renewal of it from the pabulum as follows. In the formation of the muscular fibre the mass of protoplasm moves gradually along, undergoing at the same time conversion into muscular fibre. "At the same time that it advances it absorbs nutrient matter, so that much formed material or contractile tissue may be produced without the germinal matter altering in size." (*Arch.*, IV, p. 153.)

In the secreting cells "the germinal matter remains near the lower attached extremity of the open-mouthed cylindrical cell, and takes up nutrient matter at its lower surface, while at its upper part, which forms the floor of the cell cavity, the germinal matter is changed into the *secretion* of the cell which occupies its cavity and escapes from the open orifice. A large quantity of pabulum may pass into the state of germinal matter, and a corresponding quantity of the latter undergo conversion into the formed material or secretion of the cell, while the entire apparatus neither changes in volume nor alters in form or weight." (*Oxford Lectures*, p. 665.)

It is superfluous to illustrate this point further from Beale, as, according to him, it is invariable in all formations of tissue and secretion. But I may cite an instance from another source where it was also observed in the functional action of sensiferous nervous tissue. Liebreich made the horrible experiment on dogs of torturing them till they died from prolonged and excessive pain. On analysis it was found that the side of the spinal marrow corresponding to the roots of the nerves which had been excited contained always much less protagon than the opposite side. (Gavarret, p. 241.)

As to the relation of quantity of pabulum to growth of protoplasm, it is unnecessary to quote, as it is almost everywhere insisted on by Beale to be almost in the direct ratio. On this point he goes much further than Fletcher, and accounts for the operation of mechanical irritants in this way; so this subject had better be left for the question of the stimuli.

We may now notice the most characteristic part of

Beale's system, viz. the link between the physio-chemical and the vital processes, which go on perpetually side by side in all living beings, and yet are always essentially distinct. All pabulum is described as taken into the living state, wherein its elements are held together in a state of combination entirely different from ordinary chemical combination. When growth of living matter goes on this takes place by increase and subdivision from within, and the formation of new centres of germinal matter. But when the tissues or secretions are to be formed, the elements are rearranged into the new chemical compounds and structures constituting the formed material. They have then passed again out of the living state, and he constantly uses the expression that the protoplasm dies into the particular tissue or secretion it has the specific power to produce.

For example, after describing the growing, dividing, and multiplying of masses of protoplasm, each of which is always derived from pre-existing masses of protoplasm, he says:

> "After a time some of these cease to multiply, though they still live and take up food. The living matter of which they are composed undergoes change. It dies under certain conditions, and tissue results. In this way muscle, and nerve, and fibrous tissue, and bone, and hair, and horn, and nail, and all the other tissues, are formed." (*Micros. Journ.*, July, 1870.) Or, again—"The formation of the fatty matter occurs in this way,—in the very substance of the bioplasm, but always outside and away from the new centre or nucleus, a little oil-globule makes its appearance. It results from changes in the living matter itself. A portion of this bioplasm dies, and among the substances resulting from its death are fatty matter, which, being insoluble, remains, and soluble substances, which are carried away in the blood. Starch-globules, and other secondary deposits, formed in the interior of elementary parts, are produced in the same manner by the death of the bioplasm. The fatty matter does not come from the blood as fat and deposit itself in the cell, nor is it formed by the collection and aggregation of excessively minute granules which traverse the vascular walls suspended in serum, nor is it precipitated from the nutrient fluid after the manner of crystals. But it invariably results from the *transformation of living matter*, and different kinds of living matter, as is well known, will produce different kinds of fat. The properties and composition of fat in different animals differ, because the powers of the bioplasm or living matter of each animal are so different." (Beale's *Todd and Bowman*, p. 301.)

Thus we have a very simple conception of a process which relieves us from the dilemma of Fletcher. And when we come to think of it in this light we see that it could not be otherwise. Since vitality is the result of a combination of matter held together by a peculiar affinity, which is a property, and therefore incapable of being transferred, the only way to act upon fresh matter is to take it into that state of combination—to make it a part of the compound. Any change, therein produced, by which a portion is restored to ordinary chemical combination is, in fact, death just as much as if that happened to the whole mass as in ordinary complete death.

Read by the light of Beale's theory, Fletcher's chapter on death assumes a peculiar significance. Fletcher represents death as a vital process, the last of the living actions, whereby the elements are rearranged into the chemical state; for those elements being hitherto in a totally different state, the usual chemical affinities cannot take effect on them till they are released from vital affinity by vital agency. He says of the living tissues, "their vitality cannot desert them otherwise than by a vital process" (p. 144).

But although Beale's theory removes the absolute incompatibility of two such processes as the living and the dead working together, yet it is, of course, far from being a complete explanation of the phenomena of nutrition, and many difficulties are still felt and objections raised against it. Professor Wyville Thomson thus speaks:

> "It is impossible in the present state of knowledge to subject any view as to the ultimate mechanism of the formation of tissue through the means of protoplasm to direct proof. It seems now to be a very generally received opinion, supported by Huxley, Max Schultze, Hofmeister, Beale, and many others, and notably by Oscar Schmidt, who would seem to bring it almost to demonstration in his beautiful researches on the sponges of the Adriatic, that protoplasm is simply converted, with a certain change of composition, into tissue or 'formed material.' There are, however, almost insuperable objections to this view. The secondary products of organisation (formed material) are most various in their chemical constitutions, and it involves the admission that protoplasm may change in its chemical composition till it is *almost* carbonate of lime, or silica, or starch, or horn, or

cellulose; the last stage of the metamorphosis being its absolute separation as one or other of these bodies. Another view which I have always regarded as more probable is that protoplasm, the substance which is endowed with the peculiar vital property, has always the same composition, and that it acts simply by catalysis, inducing, under certain known laws, decomposition and recombination in compounds which are subjected to its influence, without itself undergoing any change, absorbing the nascent products of combination and decomposition, and recombining them and reserving them with reference to the development or maintenance of the organ to which it gives its life." (*Nature*, May, 1871).

In the presumption that the placing of Beale's name on a level with the others is not an intentional denial of priority of the theory, no remark need be made on the position it here occupies. Nor was it necessary to adduce such a recondite example of the difficulty of the theory, as it is illustrated every instant by what goes on in ourselves and all the higher animals as well as the lower.

Beale is, in fact, here in the same difficulty as Fletcher, for he does not allow that the vital power can be taken on by any albumenoid, nor by any so-called proximate principle at all, but that before such a power can be manifested the material must be changed into a state of combination entirely *sui generis*, the passage out of which back into any ordinary chemically cognisable compound means death. But all protoplasm or living matter is nitrogenous, thus containing the four organic elements at least, besides some one or more of the fifteen other so-called inorganic elements found in living beings. How, then, can it die into a binary or ternary compound like carbonic acid or the fats or sugars, *i. e.* into substances whose elements, arrange them as you will, never could possess vitality? How are these extruded from the protoplasm while the nitrogen remains? Again, there are certain products which are synthetic. Thus, the hæmoglobin, protagon, and the probable generator of myosin, are of higher potential energy than the pabulum, and possibly also than the protoplasm; also the formation of fats from saccharine pabulum must be a reducing process; in both these processes, therefore, a certain quantum of force must be consumed and rendered latent.

Whence is this derived? We have no proof that animals can consume the active forces of heat or light for this purpose, so probably there must be some simultaneous compensatory process of decomposition of a portion of pabulum in the living acts of nutrition and secretion which has not yet been followed out experimentally. Whether any such compensatory processes will explain both difficulties must for the present remain unknown.

Beale at first (*Archives,* 1861) thought that the transition from the living matter to the formed material was a gradual one; that "if the term life and vital activity be used, several degrees must be admitted. One might say, therefore, that the vital activity of the particles gradually becomes reduced as they recede from the centre at which they became animated" (III, p. 119). But since then he has abandoned this opinion for one more consistent with his own and Fletcher's view of the constitution of living matter, viz. the following:

> "The pabulum does not shade by imperceptible gradations into the living matter, and this latter into the formed material; but the transition from one state into the other is sudden and abrupt, although there may be much living matter mixed with a little lifeless matter, or *vice versâ.* The ultimate particles of matter pass from the lifeless into the living state and from the latter into the dead state suddenly. Matter cannot be said to *half live* or *half die.* It is either *dead* or *living*, *animate* or *inanimate,* and formed matter has ceased to live." (*Protoplasm*, p. 34.)

Beale also supposes that secondary changes may take place in the formed material which may first be deposited in a more complex state of composition and then split up or be oxidated into different organic compounds (*Oxford Lectures,* 1869, p. 57). Likewise in the formation of bone he believes that the protoplasm produces the matrix, either fibrous or cartilaginous, in the same way as all other formed material, and that afterwards the earthy parts are precipitated from the nutrient fluids by a purely chemical process (*Todd and Bowman,* p. 251).

It must, however, be acknowledged that certain difficulties remain which cannot be yet cleared up. But that is no reason for rejecting an hypothesis otherwise so well grounded, and falling back into the hopeless muddle of the chemicalist school, who think to explain everything by the all-embracing word catalysis (§ 77). I think Beale may find comfort in the answer of Copernicus, who, when the objection to his theory was made that the planet Venus had no phases like the moon, replied, "Truly I do not know how this may be, but I am sure that God in his own good time will give us the means of understanding it!" Ere long the telescope of Galileo showed that the anomaly was only apparent, and dissipated the difficulty.

In the retrograde metamorphosis the protoplasm plays a similar, but now a reversed part, consuming the formed material and reconverting it into blood or into excrementitious matter. Here, however, Beale believes that oxidation and other chemical processes have a share, although that cannot be great or almost ever exclusive, for he says:

> "In every change characteristic of living beings this living matter takes a part. In the formation of every tissue, in its disintegration, in its repair, living matter is concerned. Nor does the slightest marked change take place without the phenomena occurring in the living matter of the part being modified. No formation of structure, no action occurring in the structure after it has been produced, no secretion, can be accounted for without considering what goes on in the living matter." (*Med. Times*, vol. i, 1866, p. 411.)

The last point of correspondence we may notice is that of deterioration of the living matter. After describing the specific differences of the various kinds of protoplasm which produce the different organs and tissues of the higher animals, he goes on to explain that developmental and formative power may be impaired or lost from various causes, mostly unknown. Then he says that it is a mistake to suppose that the germinative or developmental faculty is confined to embryonic life, but that there is no essential difference between the formative and nutritive operations in the embryonic and fully formed state (*Med. Times*, March, 1866). In other words, that the plastic germinal and metabolic faculties must always be present as long as life exists, but they may be deteriorated or degraded in different degrees, and more especially the germinal faculty may be degraded while the mere power of growth and increase may be even augmented, and this is a feature of many states of disease. He says:

> "But, although developmental power may be lost for ever, power of a different kind may be acquired *pari passu* during the rapid multiplication of bioplasm. Progressive advance in the capacity to form lasting structures and elaborate organs is characterised by the comparatively slow but regular and orderly growth and multiplication of bioplasm. Rapid multiplication of the bioplasm, on the other hand, involves degradation in formative power, which is at length entirely lost, never to be reacquired. Degradation in power is commonly associated with increased rate of growth, increased faculty of resisting adverse conditions, and, in some cases, such is the vitality of the living matter, it takes up the nourishment which should be appropriated by healthy parts, and these are at length starved and deteriorate, or are completely destroyed. The actively living degraded bioplasm may be capable of retaining its vitality although removed altogether and for some time from the living body, and, remarkable as it seems, it may grow and at length destroy other living organisms to which it gains access." (*Microscopic Quarterly*, July, 1870.)
>
> Again, "Increase of formative and constructive power seems to be associated with the most limited change in germinal matter, while rapid change—increased vital action—seems to be invariably connected with decadence of power." (*Protoplasm*, p. 85.)

This corresponds with Fletcher's placing disease in the secondary or lowered stage of vital action as a whole.

These principles are of extreme importance, but their bearing is so very wide that it would be necessary to discuss a large part of pathology to follow them out. On the whole, a great degree of harmony could be shown between his views and discoveries, as far as they have gone, and the principles laid down by Fletcher, but the subject can be better discussed when the stimuli have been considered. We miss, however, the reference to stimuli, the necessity of adequate exciting causes for all aberrations from health, and the therapeutic deductions, which are such prominent features in Fletcher's system. On the other hand, Beale makes a most important addition to our knowledge in the reference of the contagious principles of disease to portions of degraded, but still living, protoplasm. This, if confirmed, will prove a discovery of practical interest that can hardly yet be estimated at its proper value, and will supersede animal and vegetable germ theories of disease which have of late obtained a kind of notoriety.

This theory of the living, and yet non-parasitical, nature of the contagious principles is an advance upon Fletcher, who looked upon them as diseased secretions, acting solely as preternatural stimuli; but if the self-multiplication of degraded protoplasm is to be admitted, it does not thereby exclude the necessary co-operation of the action of miasms as morbid stimuli. Else how are we to explain the long latent stage and sudden development of the active symptoms of contagious fevers?

Likewise Beale's discoveries in respect to the true nature of suppuration have anticipated the observations of Cohnheim and Stricker, and are, in all probability, a more correct interpretation of the phenomena.

Beale's paper in the *Microscopic Quarterly* for July, 1870, and also his report on the cattle plague in the *Government Blue Book,* are of the greatest interest, and should be studied by every medical man.

§ 84. And now, after having gone so far, as it were hand in hand, I regret to find that Beale at last parts company with Fletcher. Although the former has all along insisted on the actual molecular change which matter undergoes on entering the living state, and the absence of any of the proximate principles therein, yet, to my astonishment, he after all holds that change to be insufficient to account for the new properties then acquired, and accordingly falls back upon the old vital principle. He not only rejects the physical, as contrasted with the teleological view, but rejects also vitalism in favour of animism. It is true he objects to being classed with believers in the old vital principle, but as his "vital power" is declared to be not a property of matter, nor a force, nor the result of any action, nor a phenomenon, it must be an entity, and if not the same as that of Barthez, it merely adds one more to the numerous varieties of the supposed vital principle. As expositor of Fletcher I feel compelled to vindicate his vitalism from the charge of inconsistency with the rules of sound philosophy, which certainly lies against the animism of Beale. Although, or rather because, the state of aggregation of matter and the properties thereby developed are utterly distinct in the organised state from the chemical state, Fletcher believes life to consist solely in the actions which are the necessary result of those properties. That life therefore corresponds to the Ζωὴ of the Greeks, or *Vita* of the Romans, which meant living action, and not to the Ψυχὴ, or *Anima,* which designated life as a substantial entity, which, entering into certain compounds of matter, organised them, and continued to be the efficient cause of all their characteristic phenomena. The vitalism of Fletcher is, therefore, in harmony with the physical theory of life in the abstract, and with the canon of Bacon's philosophy which emancipates science from the consideration of final causes. But it is completely opposed to numerous *false* physical theories of life, such as John Hunter's galvanic or Lamarck's compound of electricity and light, theories; and to the not less absurd theory at present prevailing, that life is nothing but certain actions produced in albumen by a force or forces correlative with heat and motion. Fletcher's chapter on the nature of life is not only the most complete and exhaustive *resumé* of the arguments against the theory of a substantial vital principle as the efficient cause of the infinitely varied phenomena of life, but it is likewise equally opposed to the admission of any teleological cause of these. They must be referred, in fact, equally with all physical phenomena, to the properties of (organised) matter acted on by corresponding powers, and therefore not to one self-sufficient substance or cause, " but to countless irrational and unconscious forces incessantly at work in every point of the system, in blind but implicit obedience to laws imposed upon them by the Supreme Being, and adapted everywhere to the end to be fulfilled " (35). And in answer to the then and still widely held opinion that the conspicuous evidence of design shown in living beings surely points to the existence of a special power acting in them with a definite purpose, he says, undoubtedly there is design, deep and wonderful, " but this design is that of the Great First Cause of all things, who has adapted in every case the physical causes—the immediate means—to the end to be fulfilled; not that of this miserable means, which acts, and can act, only in blind obedience to the laws imposed upon it. Nor, perhaps, are the evidences of design really greater in the motions, sensible or insensible, of the organic than of the inorganic kingdom of nature, nor is the employment of the terms means and end—not merely cause and effect—more appropriate in the former case than in the latter. The final causes are often no less obvious in the actions of inorganised than of organised matter. Can we behold the revolutions of the planets, the alterations of the seasons, and of day and night—

> Hunc solem et stellas, et decedentia certis
> Tempora momentis;

at one time the ascent of the waters into the atmosphere, at another its descent in rain or snow, the motions of the sea, the rivers, and the springs, the occasional changes on the surface of the earth itself, without recognising an allwise and omnipotent design? Nor are the molecular actions here also—the aggregation of a crystal or stalactite for example, or the feathery condensation of vapour—less indicative of an end in view in their operation, or of consummate skill in regulating and adapting the means to this end. In the latter class of cases, however, we unhesitatingly admit that the design is not that of the means—of the mere so-called forces of attraction and repulsion—but that of the Great Author of these forces; and why should we doubt that the same is the case in the former also?" (II, p.28.)

To illustrate farther the true physical theory of life I may adduce the testimony of the distinguished founder of the cellular theory.

Three years after Fletcher, Schwann (*Cell Theory*, p. 186) reduces the various opinions entertained with respect to the fundamental powers of organised bodies into two, viz. the teleological and the physical. In the former every organ is supposed to "originate with an internal power, which models it into conformity with a predominant idea, arranging the molecules in the relation necessary for accomplishing certain purposes held forth by this idea. And here, therefore, that which arranges and combines the molecules is a power acting with a definite purpose." "The other view is that the fundamental powers of organised bodies agree essentially with those of inorganic nature—that they work altogether blindly according to the laws of necessity and irrespective of any purpose—that they are powers which are as much established with the existence of matter as the physical powers are." It cannot be denied that adaptation to a particular purpose is characteristic of every organism, but the source of this adaptation does not depend upon each organism being developed by the operation of its own power in obedience to that purpose, "but it originates, as in inorganic nature, in the creation of the matter with its blind powers by a rational Being." The evidence of design is quite as great in the inorganic as in the organic world. "We know, for instance, the powers which operate in our planetary system. They operate, like all physical powers, in accordance with blind laws of necessity, and yet is the planetary system remarkable for its adaptation to a purpose. The ground of this adaptation does not lie in the powers, but in Him who has so constituted matter with its powers that in blindly obeying its laws it produces a whole suited to fulfil an intended purpose" (p. 187). In physics all those explanations which were suggested by a teleological view of nature, as the "horror of a vacuum" have long since been discarded. But when we speak of the inflammation and suppuration which removes a foreign body as the effort of the organism to remove the foreign body, or as a proof of the "autocracy of the organism," or of the "vis medicatrix," that is as little of an explanation, according to the physical view, as it would be to say that the motion of the earth around the sun is an effort of the fundamental power of the planetary system to produce a change of seasons on the planets, or to say that the ebb and flood are the reaction of the organism of the earth upon the moon." Finally, "when speaking of a physical explanation of organic phenomena, it is not necessary to understand an explanation by known physical powers, such, for instance, as that universal refuge electricity, and the like [he would probably now say a vague use of the word Force]; but an explanation by means of powers which operate like the physical powers in accordance with strict laws of blind necessity, whether they be also to be found in inorganic nature or not" (190). This agrees completely with Fletcher's idea of the living matter being merely a compound, although its properties are entirely *sui generis*; but as if to make the argument still more complete he adds, "As the elementary materials of organic nature are not different from those of the inorganic kingdom, the source of the organic phenomena can only reside in another combination of these materials."

He does not merely mean a vague statement that in the living state the elements, or even protein-compounds, are in a somewhat modified state of combination, but, like Fletcher, that that difference of combination is *all-sufficient* to produce the phenomena of life.

Both these authors wrote before the promulgation of the new doctrine of force, and the question now is whether that may help to reconcile the different parties by discriminating between true and false physical theories of life. At present we see Fletcher agreeing entirely with Liebig, Bence Jones, and the physio-chemical school, in the physical view, as distinguished from the animist and all teleological views, while, as a vitalist, he entirely disagrees with the particular physio-chemical theory of that school. In this last respect Beale is entirely at one with Fletcher, but refuses to admit any such thing as life without a special teleological power.

If we test the theory of the chemicalist school by the laws of matter and force applied rigidly, it will, I think, appear that it does not fulfil the conditions of a true physical theory, because the "directing idea or agency," or "vital force," which they are all compelled to fall back upon, sooner or later resolves itself into a teleological cause, either of the nature of the *horror vacui* or of a substantial principle. Unless, therefore, we admit the sufficiency of the metabolic or vital state of affinity, we cannot uphold the physical view; but if we do, then Beale's animism becomes superfluous.

§ 85. From what has been said at § 60, it must be obvious that in my opinion the explanation of vital action on the physical view must not be sought in any form of force known or to be discovered, but solely in the development of a peculiar kind of chemical affinity, called here metabolic. Likewise, as shown at § 60 and 63, the law of equivalence of force is maintained in all molecular changes, whether under metabolic or chemical affinity, because the force that unites the atoms is the same, whether determined by the property of the one or the other affinity. Just as weight is the product of the property of mass and the force of gravity, so the so-called attraction of chemical affinity may be more strictly spoken of as the product of the *property* of chemical *affinity* and the *force* of chemical *attraction.* To enable us to distinguish different genera of affinities with the same force of attraction, we may call the latter intra-molecular attraction. The tendencies of bodies, at sensible distances, to fall, or be pushed, or attracted to each other, is equal for all kinds of matter; hence mass can be determined simply by weight; but it is different with respect to intra-molecular attraction, and whether two atoms of different kinds will have the tendency to fall towards each other at all, and thus combine, and how much of "that which is expended in the production of motion" will be required in that combination, depends entirely on the specific, inherent, unalterable, inexhaustible property called affinity. When two bodies have combined, they form a new substance with new properties of affinity, quite distinct from those of the original elements; and this process of development of new affinities may go on to an extent to which we know no limit, while, at the same time, the force of intra-molecular attraction, that pushes and binds the atoms together, is the same, and can undergo no variation except that of quantity. It is therefore quite conceivable that, besides the immense variety of substances developed by the ordinary degrees of chemical affinity, including ternary and even quaternary compounds, which can be made, unmade, and made again by ordinary chemical processes, there may exist a whole genus or class of affinities produced by an atomic constitution of a complexity at present incomprehensible, and far surpassing that of the ordinary chemical compounds, and over which we could have no power by ordinary chemical means, except for destruction. This would be the metabolic or vital state of aggregation of matter. Here force would stand exactly in the same relation as to ordinary chemical compounds, and we would express this state as the product of the property of metabolic affinity and the force of intra-molecular attraction. In this state force must play exactly the same part as in chemical processes, and will be evolved as active force, or consumed in counteracting intra-molecular attraction, in proportion as the product of any transformation possesses lower or higher potential energy than its factors.

In many ordinary chemical processes no change of temperature and no transformation of force take place; in fact, no work is done, and the molecular motion required is furnished by the molecular velocities, conditioned by temperature. In like manner, many vital actions connected with growth and development, and probably perception and thought, are not work, and require no direct expenditure of force, although, as a matter of fact, every stage of them does require some expenditure of force for the molar motions accompanying them, and the internal stimuli essential for every vital action not dependent on external stimuli.

When we thus separate distinctly the ideas of property and force, and confine the latter to its strict meaning in physics, we perceive the completely subordinate, although still essential, part it plays. Force can have no power in producing, or developing, or altering the character of chemical affinity in itself. Nor has it any influence (unless for destruction) in chemical processes, except in furnishing 'that which is expended in the production of motion,' and thus giving the means of the display of the properties of the elements. Force is the mainspring of the cosmical machine, but not the works. It is in the properties of matter that lie the wonder and mystery of the universe. It is therefore superfluous to insist upon the error of attributing to force in any possible form the power of forming or building structure, or of producing development in the organic world. It is altogether an erroneous use of the word force, and it can hardly be the idea of those who have so spoken. I believe that they really mean simply to uphold the physical view of life as distinguished from the teleological; but from a loose and inaccurate use of the word force, they fall into a mode of expression, such as that the sun forms, or builds, or constructs organised beings or particular parts, &c.

Nevertheless, the language of some of the earlier authors who brought the force doctrine into physiology in this country is certainly chargeable with being the source of much of this vagueness. Here I may notice a little more in detail the essay of Dr. Carpenter, alluded to at § 61.

He first states that the characteristic of organised matter is its *germinal capacity*, which enables it to develop into its proper form and no other. If this is derived in whole from the parent, then the aggregate of all the germ forces of all the descendants, however remote, must have existed in the original progenitors, which is a *reductio ad absurdum.* Therefore " we may consider it proved that, in some way or other, fresh *organising force* is constantly being supplied from without during the whole period of the exercise of its activity." Again, " the actual *constructive force* is supplied by heat." " The heat, acting through the germ, supplies the constructive force or power by which the vegetable fabric is built up." Also, " an additional source of organising force " is to be found in the retrograde metamorphosis of organic compounds. He says of plants, that a part of the ternary compounds are brought back to binary compounds, and in thus falling to a lower plane the power consumed in their elevation is given forth in the form of "heat and organising force," which help to raise the other portions to a higher level, as is seen in germination. These expressions are very liable to mislead, although we may understand them to mean that the force is merely that which is necessary for the work done, for he says, " When we look carefully into the question, however, we find that what the germ really supplies is not the force, but the *directive agency*," so the expressions " constructive and organising" are not accurate as regards the force, which is merely used as a power guided by another, to which the word constructive and organising more properly apply. But what are we to say to this passage applied to animals?—" The source of the various forms of Vital Force—which may be distinguished as constructive, sensori-motor and generative—that are manifested in the different stages of the life of an insect, we find them to lie on the one hand in the heat with which the organism is supplied from external sources, and on the other in the food provided for it" (p. 261). Again, in speaking of the development of the higher animals, he notices the frequent retrograde metamorphosis and removal of parts, and substitution of different ones; he says, "And such removal can scarcely be accomplished without a retrograde metamorphosis, which may be considered, with great probability, as *setting free constructive force* to be applied in the production of new tissue" (p. 264).

These last I cannot explain or reconcile in any way with the nature of the physical forces, and they must have had a most confusing and misleading effect. What he here calls "forms of vital force" can be nothing but the vital, natural, and animal spirits of Aristotle and Galen, or the Anima of Harvey. No force in the physical sense could perform the functions above enumerated, and be correlative with heat and motion. Nor could any "constructive" force be set free by retrograde metamorphosis. Any force so set free could only be one of the active physical forces we know, and, although that might be used by the living action in construction, it would not be a "constructive force." Further, the "directing agency" above spoken of is not explained. Is it a property of organised matter, and is that matter only chemically combined protein; or is it a teleological cause without substance, like the horror of a vacuum, or is it merely one of those "forms of vital force" themselves which are said to be constructive and generative as well as sensori-motor, and which have their source in heat and chemical decomposition of food? Here is a confusion which I am totally unable to unravel; and I think it forms a type of the spurious "physical explanations of organic phenomena, such as by that universal refuge electricity and the like," alluded to by Schwann. These difficulties pervade, more or less, all the chemicalist theories of life, and I do not think it possible to escape them as long as we look upon the living matter as albuminates animated by any form of physical force correlative with heat and motion. All these theorists, without exception, have in the background an "idée directrice," directive agency, &c., which must be some power called force, which is incompatible with the known laws of the physical forces, or a teleological cause in the air, like the horror of a vacuum, and thus, while professing the physical theory of life, they really abandon it. I quite agree with the remark of Beale in answer to Moxon, that the supposition of any "peculiar agency" is irreconcilable with the ordinary physio-chemical theories of life (*Med. Times*, 1866, p. 631).

I can see no middle way between the metabolic change of the living matter of Fletcher (and may we say Schwann ?), as the only representative of the physical theory, and the animism of the vital principle school representing the teleological view. We must have no more halting between two opinions. Let us banish the "directive agency," the "subtle influence," and the "mysterious powers" of the present physio-chemical school to the limbo of the vital spark and subtle fluid of former days, and acknowledge that living action, growth, and development, are as much the result of the action of pabulum, conditions, and stimuli, on a peculiar aggregation of matter now called protoplasm, as the most ordinary physical or chemical process is of its particular agencies. We know not the ultimate how or why of the very simplest of these; and till we do it is idle to speculate on the infinitely complex phenomena of life, and decree that all we cannot explain depends on a special principle. Nevertheless, between the chemicalist hypothesis of albumen animated by a vital force, and the old-fashioned animism now revived by Beale, I would certainly prefer the latter, because it is at least a respectable hypothesis, inasmuch as it has in its favour the analogy of the immortal soul, which is an immaterial substance added to matter, and transmissible from parent to offspring, whereas the former rests on a mere blunder as to the nature of force. Force is, in fact, to life, no more than the organ-blower is to the musician—essential, indeed, but utterly subordinate.

§ 86. Is, then, Beale's animism superfluous ? We have seen that, with Fletcher, he believes that in the protoplasm the proximate principles do not exist, but only their elements differently arranged; that matter is never living matter when any combination into proximate principles exists, and presumably whenever the elements are arranged in the same way as they are in protoplasm, life must exist; why, then, does he require that, besides, a spiritual substance must be added to that peculiar compound in order to make it alive?

It is very difficult to find Dr. Beale's real meaning, as he has not yet given any regular and formal exposition of it, but leaves it to be gathered from incidental remarks and ironical questions, interspersed in discussions on his physiological views and in arguments against the mere chemical school. Now, in respect to this last, as we have seen, he and Fletcher are at one, so it will not be necessary to allude to them. Likewise, there is a large part of his controversial writings devoted to arguments against the physical view of nature in general as hostile to the belief in a personal, designing, superintending God, and to the Christian religion. Into these, of course, I need not enter, as they do not specially apply to our subject, nor, indeed, to physiology in particular.

The rest of his arguments may, I think, be condensed into these two positions:—1st. The difference between the phenomena of life and those of ordinary physical and chemical action is so immense that it is impossible, or at least inconceivable to him subjectively, that any combination of matter, however different from the ordinary chemical state, can develope a property like vitality. Hence we must suppose the existence of a power superadded to matter in a quite peculiar state of combination only; capable of transmission from one portion of matter to another; capable of indefinite multiplication without loss of power, and acting as a "forming, guiding, directing power or agency" on the material forces of the body, so as to produce the phenomena of life. 2nd. Such an immaterial substance or principle we know to exist as the immortal soul, and if we allow life to be possible without this or a similar principle, it would shake our belief in the immortality of the soul and all revealed religion.

From his definition of the vital power Dr. Beale does not seem to differ from the teleological theorists of the vital principle school in general, and his language bears considerable resemblance to that of Thomson, the chemist, who in

1817 speaks of the common chemical powers as "the servants of a Superior Agent, who directs them so as to accomplish always one particular end." Dr. Beale is, however, unwilling to be classed among the old vital principle school, and protests against the idea of a central guiding power, such as the Ενοϱμῶν of Hippocrates; and, in truth, his vital power resembles the *Archæi insiti* of Van Helmont, considering the specific differences of the several kinds of protoplasm. But Van Helmont conceived his several Archæi to be all subordinated to one sovereign principle, like that of the Greeks, so it is difficult to see how Beale is to account for the harmony of design of whole individual organised beings without it. It is impossible for Beale to take up any new position on this subject, for the world of thought has long been explored and mapped out, and there is no new region left for him to discover; he can merely choose in which he will take up his abode. As precision of terms is here of supreme importance, I will again quote an appropriate passage from Fletcher:

> "It is very important to keep constantly in mind the physiological distinctions between a Property, faculty, quality, or capability, a Power, stimulus, or agent, and an Action or function; the first, like irritability, sensibility, and so forth, signifying only a susceptibility of excitement; the second, like caloric, light, sympathy, &c., only a means by which this susceptibility may be called into action; while the last, like irritation and sensation, signifies the phenomena resulting from the two in co-operation. It would have appeared almost superfluous to notice these distinctions had they not been so frequently lost sight of, not only by the *οἱ πολλοὶ*, but even by authors of merited celebrity; Barclay, for instance, speaking continually of the *property* of sympathy; Bostock, of the *power* of sensation; and Adelon, of the *function* of sensibility. Life is a property with some authors, a power with others, and an action or series of actions with a third set; and the expression is in all these cases correct if in the first instance the term be used as synonymous with vitality, in the second as signifying a substantial vital principle, and in the last as indicating the sum of the functions—but not otherwise." (II, p. 5.)

According to these definitions Beale must be classed among the substantial vital-principlists, whether he will or not. There is one point, however, in which he differs from the majority of the school, but probably here he does not stand alone, as a similar view seems to have been held by a small section of the school, including Descartes, Needham, Darwin (the elder), Priestley, and others. This is that his vital power cannot enter into any ordinary chemical compound, such as albumen, but must first transform it into a combination *sui generis.* His living matter is thus protoplasm *plus* a portion of vital principle, just as Fletcher's living matter is the very same substance without any such superadded entity.

The ancients, who knew very little chemistry of any kind, did not trouble themselves about the difference between inorganic and organic chemistry, and it is only when the latter came up as a science that the difficulties began. One party boldly declares that the living parts are just the same chemical principles as we find them on analysis, but *plus* the vital principle. This, whether true or not, is at least intelligible and consistent on their view that such exists. Another party is so vague on the subject that they cannot be classified. A third admit the proximate principles, deny the vital principle, and do not allow the sole vitality of the protoplasm. This is the physio-chemical school of the majority at present. A fourth, comprising Fletcher and Beale alone, deny the proximate principles, and hold the sole vitality of the protoplasm, but they differ about the vital principle.

It is, therefore, incumbent on Beale to show why there should be a vital principle added to a substance already totally changed from the proximate principles, and therefore presumedly having totally different properties.

Among Dr. Beale's arguments the chief are the following:—He says vitality

"cannot be a property of matter, because it is in all respects essentially different in its actions from all acknowledged properties of matter." (*Protoplasm*, p. 74.)

Again, "And what are we to understand by the nature or power or property of living germinal matter? With what is it comparable? Clearly not with the properties of non-living matter, for these belong to the matter itself, while living properties are transferred from one particle to others with the utmost rapidity, and the very same matter may exist with or without its vital properties." (*Oxford Lectures*, p. 608.)

Again, "It has been argued that since the properties of water are due to its gases and not to 'aquosity,' the properties of protoplasm are due to its elements, oxygen, hydrogen, nitrogen, and carbon, and not to *vitality*. But the cases are by no means parallel. Of water there is only one kind; of protoplasm there are kinds innumerable. The constituent elements of the same particle of water may be separated and recombined again and again as many times as we please, but the elements of protoplasm, once separated from one another, can never be combined again to form any kind of protoplasm. But further, every kind of protoplasm differs from every other kind most remarkably in the results of its living, one producing man, another amœba, another dog, a fourth butterfly, and so on. . . . Have we not *identity of composition* in the living matter and marvellous difference in the results of the vital actions? How, then, can the difference be due to the ordinary properties of the elements?" (*Protoplasm*, p. 27.)

In reply to the first of these quotations, do we require to say more than that vitality is certainly different from all *other* properties, developed by all *other* combinations of matter than the one in consideration? But to say therefore it cannot be a property, is simply to beg the question at issue. To the second, if he means that the same elements remain in exactly the same proportions, and in exactly the same constitution while yet life has gone from protoplasm, this would be in direct contradiction to his whole theory that the protoplasm dies into the tissue and proximate principles. To the aquosity argument I have to oppose what has been said at § 60 to 62, and only to add that, because a simple binary compound is not susceptible of the complexity of constitution necessary for the production of vitality, is that any reason why a compound of five or six elements and an enormous number of atoms should not be so? If two letters of the alphabet are insufficient for the wants of human thought, is that a reason why twenty-four should be? But we may turn the argument the other way, and ask why such a marvellous thing as the vital principle can *not* confer vitality on binary or ternary compounds? Again, we can make, unmake, and make again the chemical compounds, but we can neither make originally nor restore when destroyed any particle of living matter. Granted the last clause, but the former is not unconditionally true; we can neither make nor restore the diamond: is there, therefore, a diamond-anima? The same may be said of the vast majority of organic compounds, with which it is the rule and not the exception that, although you can destroy them all and make some out of others, yet you cannot make again one that has been destroyed.

The asserted identity of the protoplasm would be a most powerful argument, but on looking at that closely we see it rests on a pure assumption. We have as yet no comparative analysis of the different kinds of protoplasm, and it must be extremely difficult to obtain such from its being so intimately interwoven with all the tissues; but as far as the testimony of organic chemistry goes the discrepancies and imperfections in the total percentage of analysis of different parts would lead us to presume the existence of difference in the dead products. And no method can as yet give us the least information as to any difference of atomic constitution in the inconceivably complex state of the living molecule. The only identity or unity we know is the anatomical one, which restricts vitality to the clear structureless stuff called protoplasm, but that may possess an infinite variety of metabolic constitution. Upon what is thus a pure assumption, see what a fabric Dr. Beale builds! The objections to the doctrine of a general vital principle, which, like the widow's cruise or the five loaves, can be multiplied infinitely in transmission from parent to offspring, and then annihilated at the death of each individual, have been sufficiently insisted on. But those applying to Beale's revival of the *Archæi insiti* are even more cogent. For if all protoplasm is identical, its metabolic composition can contribute nothing to the specific differences obtaining between different kinds of protoplasm. These can only depend on varieties of Archæi possessed by each; and not only must each species be represented by its specific Archæus, but each organ, and even each part of each individual, must have its own variety of Archæus. Likewise with respect to those degraded forms of protoplasm whose recognition may add additional lustre to Dr. Beale's name, are we to suppose the degradation resides in the substance of the Archæus in analogy with the moral degradation of the human soul by sin? Or that, besides the Archæus of health, the protoplasm possesses a variety of Archæi of a lower type in a dormant state, ready to be roused into activity by an exciting cause, and thus give rise to pus, cancer, tubercle, and the like? Surely it is unnecessary to pursue farther the contrast between this complicated system of entities and the simple view of Fletcher, that, as all vital properties depend on the metabolic composition of the protoplasm, so all deteriorations depend on mere alterations of that composition. We have so far seen no argument against the true physical theory of life except the enormous difference in kind between the merely physical and the vital properties, and as we pursue the subject we find substantially the same argument reiterated in various forms. Under the head of "self-constructing properties of molecules" he speaks of the physical theory here given as if that implied that the mutual interaction of the properties and forces of matter must necessarily develope the metabolic state spontaneously, or, in other words, as if "spontaneous generation" were a necessary part of it. In opposition to that I need only refer to § 63 (p. 112), where Fletcher's opposition to the theory is quite as strong, and founded on exactly the same reasons, as Dr. Beale's. The miraculous interference of the Almighty is recognised quite as strongly as by Dr. Beale, but it is allowed to be a legitimate subject of speculation whether He then created a new spiritual entity or added new properties* to matter, or merely *fashioned* the then existing matter into a compound which, in virtue solely of that fashion, acquired the new properties of self-reproduction from heterogeneous particles which is the main characteristic of life. Dr. Beale, I must be allowed to say, rather dogmatically insists that we must believe that a vital principle was then created under penalty of being called infidels, but I rather think the last of the above modes is more consonant with the language of Holy Writ, if he will insist on bringing that into a scientific question.

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* I do not think we have any grounds for supposing that special properties were given to the four so-called organic elements which specially fitted them for life. We must remember there are fifteen other elements which take part in the structure of organised beings in our planet, and these may, on the protoplasmic theory, all have passed through the living state, for however brief a period. There is no reason to suppose that the remaining elements might not take part in the living structures, and contribute each its peculiar modifications to the metabolic state, if required for the conditions of life in our planet. We must therefore suppose that the potentiality of life exists in all the elements. Not that all inorganic as well as organic actions are to be

Again, in spite of Dr. Beale's vehement protest against the worn-out crystal argument, I would repeat the question, why should not form spring in living things as well as in dead from the operation of causes acting in blind obedience to certain laws? A formless germ gradually grows, differentiates, and finally settles into a form of wonderful beauty, harmony of design, and adaptation to a purpose, in which every particle finds its appropriate place prepared for it long before a millionth part of these particles had any existence as such. But does not the same take place in the crystal? The individual particles of it have no power over the ultimate shape except as arranging themselves in blind

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considered a kind of life, as the ancients supposed, but simply that the elements have all the potentiality of taking part in metabolic compounds, just as oxygen and hydrogen have none of the properties of water, or of an abstract aquosity, but it is owing to their inherent properties that their compound has the properties of water. In the question of the plurality of worlds there has been apparently a too exclusive reference to the conditions of life in our planet. The essential conditions of a protoplasm, or of a metabolic state of matter, seem merely to be a certain uniformity or limited range of variation of temperature, so that some of the elements capable of manifold combinations with absorption or evolution of force, and in the state of solids, fluids, and gases, should exist. Within the ranges of absolute temperature in our planet it would seem that those best adapted for this are C, O, H, N, with some inorganic ingredients. These, therefore, must appear to us *par excellence* the organic elements, and to have some special aptitude for life. But it is not difficult to conceive that the metabolic state might be taken on at far other temperatures, say below the freezing-point of mercury or at the temperature of the sun's heat. But, of course, quite different elements would then play the part of solids, liquids, and gases, and extricate force by their combinations. It would not be difficult to construct a possible table of substances capable of fulfilling vital functions at very different temperatures from this of our planet.

obedience to the action of certain forces on their inherent properties. Is the result less beautiful or harmonious, or adapted to a purpose, and thus indicative of design on the part of the Creator? And yet does anybody imagine the existence of a teleological, prototypal, formative "power," which shall guide the particles into their places? Nor can there be any such thing as a "force" of crystallization; that is another loose and inacurate use of this much abused word. To this Dr. Beale answers by enumerating the immense differences between a crystal and a mass of protoplasm, which have here been granted over and over again in every form of superlatives; but how does that touch the question? For if Dr. Beale will turn to any part of his own physiological writings where he describes the specific varieties of protoplasm—say, for example, that of bone, quoted here at § 83, p. 175—he will find that bone protoplasm, like many others, if taken from their natural position and supplied with appropriate pabulum, stimuli, and conditions, will still go on forming bone, or what not, in blind obedience to its properties and the external causes, simply because it cannot help forming these tissues, although it may be to the death and destruction of the individual. In like manner the protoplasm of the eye or of the heart form these organs, marvellously perfect and adapted for their purpose as optical and hydraulic instruments, in blind obedience to the laws of their constitution, in virtue of which matter associated into their composition and acted on by the usual conditions cannot help making eyes and hearts. And if we go a step back, viz. to the germ, we find there a kind of protoplasm which cannot help differentiating into other kinds of protoplasm, which shall in turn make parts, organs, and tissues. Does it matter for the general question whether that obedience was paid to the fashion of the material particles or to the power of an equally blind and irrational fragment of Archæus? The evidence of design through the operation of blind and irrational forces and properties is thus exactly the same in the organic as in the inorganic world, only the processes in the case of the former are more complex and difficult for us to follow. It would be easy to pursue the subject to a greater length and to show that Dr. Beale's objections from the protoplasmic movements, the want of molecular machinery, the impossibility of matter guiding forces, and similar arguments, can be reconciled with the physical theory, and if not, that certainly neither they nor the previous ones prove that a vital principle could exist and be capable of functions such as are ascribed to it. The one sole and solitary argument for its existence is analogy with the immortal soul.

§ 87. But all, literally all, that we really know of the existence of the immortal soul of man is founded in faith in the revealed word of God in Scripture, and I entirely agree with those who protest against bringing arguments from that source into a scientific question. I am content to take religion wholly from the Bible, and science wholly from nature, in perfect trust that ultimately, when the interpretation of the Word of God is fully understood and when nothing remains to be explored in science, the harmony between the two will be manifest. Before that time I am satisfied that all attempts to bring them forcibly into harmony will remain, as they have been hitherto, injurious and derogatory to religion and destructive to the progress of science. In particular the wish to support our faith in the immortality of the soul by calling in the aid of the heathen idea of a vital principle is here to be objected to. For it is nothing but an idea of heathen origin. If we trace it back to its source we find that Beale's vital power differs in no way from the immaterial entity of Alison, Kirby, Bell, Prout, Barclay, and others of the last generation, nor in any essential particulars from the Principle, Archæus, Anima, or what not, of Barthez, Whytt, Stahl, Van Helmont, Harvey, and others of former generations. Beale is, in fact, perhaps, most nearly represented by Barthez and Van Helmont. But there is no essential difference between all these and the philosophers of the pre-Christian period. In fact, the vital principle of the moderns is, as Fletcher says, just the same thing or the same nothing as the Πνεῦμα of Galen, after Chrysippus, the Ψυχὴ of Plato, or the Φύσις of Pythagoras, who borrowed it from the Egyptians, and they in turn from the Brahmins of India, with their Elementary Fire or the Soul of the World, a portion of which—a *divinæ particula auræ*—was supposed to be imparted to every living creature and constitute its proper life. Thus, the vital principle rests on, not only a heathen, but a pantheistic idea. Dr. Beale here treads delicately. No one supposes him to hold the pantheistic notion, but he nowhere, that I can find, distinctly states what becomes of his "vital power" at death, so I assume him to mean that it is annihilated. It is, therefore, a species of mortal soul, and why that which passes away as completely as the metabolic fashion of each portion of protoplasm should lend support to our faith in the immortality of the human soul (if any support is needed from such a source), I am at a loss to perceive.

The truth is, the false use of the word materialism is a mere bugbear although it certainly has still some influence against the physical theory even in the minds of the learned, while it largely influences the numerous half-instructed readers of the semi-popular literature of the day. I have no pretensions to argue this or the full question involved in the bearing of science upon spiritual religion as revealed in the Scriptures, but I shall content myself by recalling the deliverance on the subject before us of a man of large and philosophic grasp of mind like Fletcher, who was a member of the Church of England, and whose orthodoxy was never, as far as I am aware, called in question.

"There is in the minds of many persons, not only among the uninformed, but also among the educated, a vague indefinite kind of impression that the vital principle, the sensitive principle, the rational principle, and the immortal principle, are all identical; and that he who denies the substantiality of the first does the same with respect to all the rest. This impression appears to have originated in the ancient complicated absurdity of applying to the three supposed principles of life, of sensation and of thought, and to the one real principle of immortality, the same name as soul, spirit, and so forth." (II, p. 32.)

"That the soul is something entirely independent of either a sensitive or a rational principle will be as far as possible demonstrated in future; and that it is independent also of a vital principle must in the mean time be obvious to any one who considers for a moment that this latter principle is ascribed indiscriminately to every organised being—a quadruped, a bird, a reptile, a fish, an insect, a worm, a zoophyte, the lowest fungus—while a soul is imputed to man alone. The differences in the vital phenomena displayed by the most abject tribes of organised beings and by man are only in a degree; in this respect *Faba est cognata Pythagoræ;* the principle which we concede to the latter as the cause of these phenomena must be conceded equally to the former; and if in the one case this principle "Redit in nihilum quod fuit ante nihil," if it be in the one case "the be-all and end-all," it must be so in the other also; so that to connect man's hopes of immortality with the admission of a vital principle within him is so far from favorable that it is directly opposed to all the best persuasions of religion, natural as well as revealed.

"But the hopes and expectations of man are founded, or should be founded, on a basis infinitely more sure than the supposed existence within him of any such principle—on a basis proper to him, not common to him and the vilest worm or weed—on the suggestions of his reason and on the strength of his faith, and as so long as these actuate him every other evidence of the existence of a soul must be superfluous, so were these once withdrawn every other evidence must be nugatory." (II, p. 33.)

"Nor is this view of the matter, as is sometimes vaguely supposed, in any degree hostile or inconsistent with the purest and loftiest religion. The hackneyed arguments against this opinion, founded upon its supposed immoral tendency and impiety, appear to proceed upon the principle, certainly erroneous, that the mind and soul are identical. Who that has watched for five minutes the action of a dog can be so blinded as to deny that he possesses attention, imagination, abstraction, judgment, desire grief, in short all the intellectual faculties and passions in the, display of which *thought* consists; but who will attribute to him an immortal *soul?* The existence of such a substance, attached during life to the body of responsible man, and surviving him to all eternity, we are at once intuitively led and explicitly taught to believe; but it is a question of morality and faith, not of physics and demonstration." (III, p. 93.)

"'I have no hope of a future existence,' observes the late talented Regius Professor of Divinity in the University of Cambridge, 'except that which is grounded on the truth of Christianity;' and it was well remarked lately, 'That if man be not satisfied to place his hopes of immortality in a Divine gift, he must confess that the difference between his own claims and those of many other animals is in a degree only, and that degree in some instances a very small one.'" (III, p. 94.)

"As often, then, as it shall be said that mind, or the faculty of thinking, is a property of living matter, as much as irritability or sensibility are properties of it—that it is born with the body, developed with the body, decays with the body, and dies with the body—it is to be understood to mean the mind only, and not the soul. The soul is certainly something not material, indeed, but substantial, a Divine gift to the highest alone of God's creatures, responsible for all the actions of the mind, but as totally distinct from it as one thing can be from another." (III, p. 95.)

It may be thought presumptuous in me to enter the lists against one who has made such great discoveries as Dr. Beale, but as expositor of Fletcher I am compelled to do so, because, as before said, they are the only two who hold the residence of life solely in the protoplasm, and I am bound to protest against the presumption of animism as necessary to that theory. For my own part, perhaps, I may be allowed to express my belief that Dr. Beale's reversion to animism is only a temporary revulsion from the gallipot pathology of the mere chemicalists to which he expresses so much aversion. I can hardly think he has arrived at the matured stage of thought on the subject, especially as we see that on one point, viz. the argument from the protoplasmic movements he makes a mere slip which he will doubtless correct ere long of himself. I am not without hope that if he is induced by the coincidence of Fletcher's theory with his own to study the system of the former, he may be led to abandon his temporary reversion to hypotheses which are the fruit of the subtle imaginations of the Greeks and their imitators the schoolmen of the middle ages, and return to the masculine philosophy of Bacon and Newton.

ERRATA.

Page 21, lines 19 and 23, *for* grains *read* grammes.
,, 22, line 20, *after* specific heat, *add* "of air."
,, 58, note, *for* 2111 *read* 211.
,, 70, *for* Bernouilli *read* Bernouilli.
,, 74, line 7, *after* downwards, *insert* "so."
,, 86, line 13, *dele* "see appendix."
,, 101, line 28, ditto.
,, 104, line 20, *for* no, *read* not.
,, 107, lines 20—21, *dele* "living matter of the."

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